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THE

Psychological Review

EDITED BY

J. MARK BALDWIN, Johns Hopkins University AND HOWARD C. WARREN, Princeton University

CHARLES H. JUDD, Yale University (*Editor Psychological Monographs*).

WITH THE CO-OPERATION FOR THIS SECTION OF

J. R. ANGELL, University of Chicago; A. C. ARMSTRONG, Wesleyan University; C. M. BAKEWELL, Yale University; ALFRED BINET, École des Hautes Études, Paris; W. L. BRYAN, Indiana University; WILLIAM CALDWELL, McGill University; MARY W. CALKINS, Wellesley College; JOHN DEWEY, Columbia University; C. LADD FRANKLIN, Baltimore; H. N. GARDINER, Smith College; G. H. HOWISON, University of California; P. JANET, Collège de France; JOSEPH JASTROW, University of Wisconsin; ADOLF MEYER, N. Y. Pathol. Institute; C. LLOYD MORGAN, University College, Bristol; HUGO MÜNSTERBERG, Harvard University; E. A. PACE, Catholic University, Washington; G. T. W. PATRICK, University of Iowa; R. M. WENLEY, University of Michigan.

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The Psychological Review.

A STUDY OF THE EARLY USE OF SELF-WORDS BY A CHILD.

BY PROFESSOR CHARLES H. COOLEY,

University of Michigan.

It has long seemed to me that the first use by children of names for the self — particularly the pronouns 'I,' 'me,' 'my' and 'mine' — was a matter of peculiar interest. Here, if anywhere, I thought, we may hope to make out what the self-idea actually is, in its naïve and comparatively simple form, in the form under which it functions in the every-day relations of life. I was especially attracted by the interest of the matter for sociology — as throwing light on the question how far and in what sense the self-idea is a social conception — but I suppose it has a bearing on other aspects of psychology, and perhaps on metaphysics.

Having already made some scanty observations in the development of two of my children[1] I determined, in the case of a third, to give especial attention to this point and make as full a record as practicable of the use of self-words and whatever seemed to bear upon it, up to the time when they were thoroughly mastered. This determination was not carried out with ideal completeness; but I hope enough observations were made to be of some value.

The record extends from soon after birth until the thirty-third month, when the normal use of self-words seemed to have been acquired. The method was simply that of ordinary observation, with very little deliberate experiment: the idea being to get a view of development under usual conditions.

[1] See the chapter on 'The Meaning of I' in *Human Nature and the Social Order*, 1902.

The observations relate chiefly to a period of about a year, beginning with the twentieth month, during which the child 'learned to talk.' Some scattered notes, however, are given from an earlier period, as being possibly suggestive of the growth of the self-idea before it becomes articulate. The matter given, with the exception of that in brackets, consists exclusively of verbatim transcripts of notes made at the time the phenomena were observed. It must be said, however, regarding the theoretical suggestions thrown out in the notes, that I had some theories to start with.

I proceed to state more particularly some of the questions in relation to which the observations seem to me to be of interest.

The General Problem. — This may be said to be, "How is 'I' learned and what does it mean?"

It is evident that the learning of 'I' offers a somewhat peculiar problem. The reason is the apparent impossibility of learning its proper use by direct imitation. As used by other persons it never, apparently, means the same thing as when used by the child. An apple is an apple to all alike, but 'I' is different for every user of the word.

As to what it means; the following, to me at least, are interesting questions. Does it mean the physical self? If something else, then what? How far, or in what sense is it a social idea? How is the naming of the self-related — as to priority and otherwise — to the naming of other persons? What other names for the self are there, and how are they related to the pronouns of the first person? On all these questions the notes have more or less bearing, which I will try to indicate, in part at least, with the hope of rendering them more interesting and intelligible.

Inarticulate Self-feeling. — The child early manifests a feeling akin to pride in control over her own body and over material objects. (See observations on the fourth month, twentieth day, on 10–16, 11–22, 15–17.)

This extends, nearly or quite as early, to the sense of power over other persons. (3–1?, 3–4?, 3–6?, 7–1, 11–22, 13–28, 15–8, 15–17, 15–24.)

The Correct Understanding of 'I' and 'You' when Used

by Others. — This was achieved by the middle of the nineteenth month. (19–14, 19–16, 19–19.) It is to be noted that this does not involve the same problem as the correct *use* of 'I,' since that word may be understood as a mere objective name for the other person.

Imitative Use of 'I' Phrases, Apparently Without a Sense of the Subjective Reference, or Indeed any Discrimination of 'I' from the Rest of the Phrase. — This is clearly the first step in the actual use of the word, and is a phenomenon that continues, more or less, long after the correct use is acquired. It began, in this case, early in the twenty-second month. (22–7, 22–12, 23–1?, 23–17, 23–23, 24–3, 24–6, 24–7, 24–8, 24–9, 24–10, 24–12, 24–15, *et seq.*)

Suggestions as to How the True or Subjective Meaning of 'I' is Grasped. — This is in some ways the most searching question of all. My answer is that the child gradually comes to notice the indications of self-feeling (the emphasis, the appropriative actions, etc.) accompanying the use of 'I,' 'me' and 'my' by others. These indications awaken his own self-feeling, already existing in an inarticulate form. He sympathizes with them and reproduces them in his own use of these words. They thus come to stand for a *self-assertive feeling or attitude*, for self-will and appropriation. This view is reinforced by noticing how responsive the child is to stress, and how readily she understands and reproduces exclamations and other emphatic words. (14–9, 21–13, 23–5, 23–19, 23–21, 23–23, 24–6, 24–10, 24–15, 25–4, 25–5, 25–6, 25–13, 25–21, 26–2, 26–13, 26–16, 26–17, 26–28.)

Examples of the Earliest Correct Use of 'I' and Other Pronouns of the First Person. — (23–1, 23–5, 23–12, 23–21, 23–22, 23–23, 23–30, 24–4, 24–6, 24–12, 24–15, 24–16, 24–18, 24–20, 24–21, *et seq.*)

Does 'I' Mean Primarily the Visible or Tangible Body? — To this, if I correctly interpret the observations, the answer must be, No. 'I' means primarily a self-assertive feeling, linked with action or emphasis expressive of the same. The earliest examples connect it with the assertion of sensation, of action, of service and of appropriation ('I see mama,' 'my

toast,' 'I make go,' 'I carry pillow,' 'I get it for you,' 'my mama,' 'I go play sand-pile,' 'I got two flowers,' etc.).

Moreover there is another name which comes into use about the same time as 'I' and means primarily the physical body. This name, in this case, was 'baby,' and was apparently learned by direct imitation and association, like the name of any other visible object. The shadow on the wall and the reflection in the looking-glass play an important part here. It is noteworthy how comparatively late self-feeling seems to connect itself with these images. They are much less interesting at first than the shadows or reflections of other persons. (19–16, 19–23, 20–25, 21–13, 23–2, 23–7, 23–14, 23–19, 23–25, 23–28, 23–31, 26–2, 26–28.)

In what Sense is 'I' a Social Conception? — The answer to this is apparently something as follows: 'I' is social in that the very essence of it is the assertion of self-will in a social medium of which the speaker is conscious.

A sympathetic study of the early use of the word will, I think, make this quite plain. 'I' is addressed to an audience — usually with some emphasis — and its purpose is to impress upon that audience the power ('I make go'), the wish ('I go play sand-pile'), the claim ('my mama'), the service ('I get it for you') of the speaker. Its use in solitude would be inconceivable (though the audience may, of course, be imaginary).

To put it otherwise, 'I' is a differentiation in a vague body of personal ideas which is either self-consciousness or social consciousness, as you please to look at it. In the use of 'I' and of names for other people, the *ego* and *alter* phases of this consciousness become explicit.

Others are Named Before the Self. — This was clearly true in the present case. (14–26, 15–8, 18–30, 19–16, 19–18, 19–23, 21–13, 24–20.)

Other Names for the Self. — It is well known that little children frequently use a name for themselves that is directly imitated from others, like the name of any other object. In this case as already noted the first name of this sort was 'baby.' It did not precede 'I' and gradually merge into it as the child be came more sophisticated (many have the impression that this is

what takes place), but it arose simultaneously, so nearly as could be ascertained, with the pronoun form. (23–19, *et seq.*)

'Baby' meant at first her reflection in the glass, her shadow on the wall, her physical person. (19–16, 19–23, 20–25, 23–7, 23–14, 23–19, 23–28, 23–31.) It was soon, however, applied to action and possession. (23–25, 24–15, 24–20, 24–21, *et seq.*)

It gained on the pronouns and for some weeks was the commonest name for the self, though it never entirely supplanted 'I.' (24–21, 24–25, 25–2, 25–14, 25–16.) The pronouns, however, mended their pace, gained a lead, and gradually displaced 'baby' altogether. (25–21, *et seq.*, to the end.)

'She,' another self-name acquired by direct imitation—*i. e.*, by hearing people say 'she needs a clean dress' and the like—was used as early as 23–2 and is noted as common towards the end of the twenty-sixth month. It continues so for about two months, then diminishes, and is discontinued about the end of the thirty-second month. (26–22, *et seq.*, to the end.)

The use of 'you' for 'I' by direct imitation (*e. g.*, 'I carry you,' meaning 'you carry me') is first recorded at the end of the twenty-third month, and remained common well into the twenty-seventh. I think, however, that this was never so definitely and deliberately used as a self-name as was 'she'—perhaps because 'you' was already understood in its true sense. Its use for 'I,' though very common, was mostly confined to its occurrence in phrases which were repeated as wholes.

The observations follow.

Second month, 11th day. *B* cries for her bottle, and cries in apparent anger when it is taken away from her. Sometimes cries when laid down.

3–1. *B*, two months old, already cries to be taken up, or in protest against being laid down, also for her bottle. [This and some of the following observations are given as possible manifestations of an early sense of appropriation, such as later underlies the use of self-words. Those cases where the thing appropriated is not a material object but the attentions of other people are especially pertinent. They seem to be the beginnings of that desire for control over others, for social power, which plays so large a part in the mature self.]

3–4. Apparently she has a sense of being neglected, which makes her angry. If I let her simply lie in my lap she screams angrily, but picking her up and changing her position mollifies her. It is not pain that makes her 'mad.' If so she already appropriates attentions that do not minister appreciably to her comfort, has a social self. [Perhaps mere discomfort sufficiently accounts for the crying.]

3–6. *B*, laid down on the bed yesterday, cried hard, with intervals of rest, for twenty minutes or more. *E* finally took her up. She was 'mad.'

4–20. *B* has a look of power and triumph — eyes open and lips pressed together — in grasping and shaking a napkin-ring, or my beard.

7–1. *B*, six months old, clearly appropriates *E*'s attentions. She is used to them and has the my-feeling about them. She cries 'mad' when she is laid down on the bed and *E* goes away.

10–16. What pride *B* shows in her face when she stands on her own legs, holding on to the chair-back, or makes the water splash in her tub. She has put forth will, has intended and achieved, and her look shows the sense of power.

11–22. *B*, nearly one year old, is playing with a large envelope. She pulls it away and grunts angrily when I take hold of it. When she is standing on a chair and I am holding her by the dress she will sometimes try to remove my hand from the latter [*i. e.*, resents assistance]. When *E* or I have been with her and attempt to go out of the room she generally cries.

13–2. *B* now knows 'your ear' [*i. e.*, will touch her own ear when she hears this expression]. She has had her hand directed to her ear, also watched herself feel of it in the glass. For a week or two she has known 'ear' as applied to others.

13–3. She does not remember 'your ear' to-day. She imitates a good deal by ear, little by eye. A hissing sound she repeats when she does not [immediately] hear it made.

13–9. She distinguishes vaguely between 'ear' and 'your ear,' — not, I think, with any [true] sense of 'your.' 'Your ear' is merely a name for certain sensations.

13–28. *B* loves to walk holding my finger. She screams determinedly when I sit down, or when *R* or *M* is offered as a

substitute. She will also lead me to the front door and insist on having it opened and going out. In general she is capable of forming clear wishes and insisting on gratifying them.

Her 'self-consciousness' or 'showing-off' idea is not yet very distinct, though *E* thinks she is often conscious of spectators.

14–9. *E* was feeding baby alternately with herself orange at the table and saying with some emphasis 'now it's *my* turn.' So the word might easily be learned. [*I. e.*, the self-assertive emphasis and feeling on the word 'my' might be learned along with the sound, and the child thus acquire an inkling of the true meaning of the word as the name of a self-assertive attitude.]

14–26. *B* seems to take no interest in her reflection in the glass. Has she become used to it as a picture and not sophisticated enough to care for it as herself?

15–8. *B* has, I think, no self-consciousness regarding her reflection in the mirror. She will smile at it as at another baby, or look puzzled; but there is nothing that suggests self-recognition.

She will sometimes pluck my sleeve or foot to get a look [from me] when I am reading.

15–17. *B* is very vigorously appropriative; screams when she is not allowed her own way, as when she wants to sit in her mother's lap or to have a plaything. Is said to have objected the other day to Emily (still younger) touching her playthings.

15–24. *B* seems to be jealous, or at least fretfully appropriative, of other people. She is not well; her teeth irritate her and her digestion is out of order. She will not let any one else touch her mother when the latter is holding her, but will grunt complainingly and push the offender away. So when I had her at *T's* she objected to their baby touching me. She expresses the idea 'mine' very clearly.

15–27. *B* is very fond of being talked to — more and more evidently so for some weeks past. She makes noises in reply, chiefly a soft ah, apparently intentional.

15–30. *B* has been indisposed for some weeks, 'out of sorts.' She frequently objects to being looked at, especially by her mother, and will complain and push the other's face about

till it looks away. Perhaps she feels the anxiety [expressed in the face] but I think it is a 'sense of other persons,' a feeling of disturbance and claim.

17–. She imitates dog, cat, squirrel, horse, whistle of nipple, squeaking of finger on tub, children crying, etc., bluejay, rushing of water, piano, pounding of hammer, tearing of paper, many human sounds. The more familiar used as signs.

18–2. *B*, seeing a picture of a man showing his teeth — a tooth-powder advertisement I believe — shut her jaws and opened her mouth in imitation. This is the first conscious facial imitation I have noticed. She watches herself in the glass occasionally.

18–30. *B* shows no (reflected) self-consciousness, nothing like vanity or affectation. She seems to attach no particular meaning [?] to her own name, though often called by it. She betrays no interest in her own shadow on the wall, though she recognizes 'mama,' 'papa' and occasionally *R* [on the wall].

19–14. *B* apparently understands 'my' and possibly 'me.' With four of us sitting about she will correctly put her finger on 'your' (*i. e.*, her own) eye, nose, ear, tooth, etc.; 'my,' '*R's*,' 'mama's,' 'papa's.'

19–16. *B* seems to have no reflective self-consciousness. She has and uses names for papa, mama, etc., none for herself. It is doubtful if she understands any name for herself, though she knows the meaning of 'your' as applied to nose, ear, etc. She recognized and named our shadows on the wall before showing any interest in her own. She knows the latter now as 'baby,' but seems to regard it merely as an external object. [She understood but did not *say* 'baby' as referring to her shadow.]

19–18. *B's* words are used when she sees the object, or when some one mentions it, not deliberately to ask for something. 'Mama' she has only begun to use in this way within a few days, *i. e.*, calling out when she sees *E*.

19–19. 'Other.' She understands this perfectly; 'other eye,' etc. When I said 'Where is the other mouth?', she pointed to *R's*. She does not know 'his' or 'her' though clear on 'my' and 'your.'

19–23. *B*, as *E* and I agree, has no reflective idea of herself at all. She knows the reflection in the glass and the shadow on the wall as 'baby' but seems to view them merely as external objects, without self-feeling. She is beginning to call the rest of us by name, but has no name for herself.

B has none of the evident 'self-consciousness' that *M* had much earlier, none of the 'perfect little actress' tricks. She is learning to talk rapidly.

20–25. *B* when asked 'Who is this?' (touching her on the chest) answered, 'baby.' This is the first time she has been observed to name herself.

21–13. I notice that *B* constantly hears 'my' and 'mine' used emphatically and addressed to herself. For instance, at table her mother, feeding her, will say of the food 'no, this is mine,' etc.

The children try to teach her to say 'baby,' but as yet she does not do so at all of her own accord, though she says 'mama,' 'papa,' and 'Leila' freely.

22–7. *B* now says 'I see' or 'I do' imitatively, with no real sense of the meaning of 'I,' after *E* or the children. To-day she said 'two ear' the first observed case of her making a sentence, limiting one word by another.

22–12. *B* has been taught to say 'I see Yacky,' 'I see papa,' etc. She seems to have some inkling of the meaning, and will say 'I see cocky' to the picture. It is all imitative and probably involves no true sense of 'I.'

23–1. *B* was playing by herself in the room with *E* to-day, and began to say 'my mama' over and over, meditatively. Presently she changed to 'my papa,' then 'my hanky.' She said it very plainly. It is not apparent why she connected it [*i. e.*, the word 'my'] with these words. Doubtful also whether she had any sense of the meaning.

23–2. She still has no word for herself. We call her 'baby,' but she never uses it. . . . If you say 'Where's baby?' or 'Where's *B*?' she says 'Here she is.' [This last phrase probably a mere echo.]

23–4. It is hard to tell whether *B* appropriates her reflection [in the glass] or not. Sometimes she seems indifferent, some-

times a little 'strange,' as if it were some child she did not know. She was much amused and delighted when she saw it with its clothes off. Never, apparently, does she seem 'self-conscious' about it, like an older girl 'seeing how she looks.'

23–5. *E* says 'I see baby.' *B* says [in answer] 'I see mama' *not* 'I see baby.' She even seems, sometimes, to emphasize 'I.' Of herself she said 'I see O' [the letter]. She knows 'I see' as an exclamation of seeing; but whether she feels the self-assertion in it I can't say.

23–7. *B* understands that 'baby' means herself when someone else speaks it (probably also 'Mary Elizabeth' and 'Mary') but she will seldom or never say it herself. Touch *E* and ask 'Who's this?' she says 'mama': touch me, 'papa'; touch her, she remains silent or grunts. She will not repeat the word 'baby' after you in any connection, though she will almost anything else. She does not seem embarrassed or self-conscious or puzzled; merely prefers not to use the word. It is hard to interpret it, I can only suppose some slight embarrassment. Formerly she did say it occasionally, now not at all.

23–12. *E* thought she said 'my toast' this morning, but could not say whether she really meant it. It is easy to see, however, how she can learn 'my.'

23–14. Proofs from photographer. *B* will point to her own picture when we say 'Where is baby?' but seems quite indifferent to it and will not name it when we point to it. On the other hand she eagerly points out all the rest of us.

23–17. *B* is perfectly able to say 'my' as indeed almost anything else, and occasionally does so imitatively. Just now *E* said 'Where's my shoe?' and *B* repeated 'my shoe.'

23–19. This evening *B* overcame her reluctance to name herself. *E* touched herself saying 'Who's this?' *B* said 'mama.' Then she touched *B*, repeating the question. *B* answered 'baby' with a smile — almost a laugh. She has often refused to do this, though she knows the word 'baby' very well.

I notice that *B* — and doubtless all children, *vide R* — quickly notice and imitate any word used with strong emphasis, reproducing the emphasis as exactly as possible. To-day *E* lost her patience over the push-cart and said 'the deuce' half under

her breath. A few minutes later *B* began saying it, repeating it many times. This may bear on the learning of 'my' and 'I,' being words often brought out with strong feeling. 'My' might be used at first as a mere exclamation associated with controversy.

23–21. *B* was calling her ball 'come ball,' then 'mama.' *E* said 'don't call me, I'm not a ball.' Immediately she began to say 'Don't call you, don't call you.'

B this morning was pulling the thread from some of the spools on *E's* screen, making the spools revolve. She said 'Makee go, makee go.' Presently she hesitated and said 'I makee go, I makee go' quite clearly, kind of singing it to herself. She has never used 'my' understandingly nor called herself by any other name [?]. N. B. Just as I was saying this in her presence she remarked 'My papa' as if to confute me.

[The above is the first fairly clear case of the deliberate use of 'I' with a true sense of its meaning.]

I have little doubt that *B* uses 'I' with a sense of appropriative feeling which she has perceived to accompany its use by other children. *M*, for instance, is constantly saying 'I'll do it,' 'I'll show you,' etc.

23–22. *E*, "Where is the marble? Do you see the marble?" *B*, 'I see marbly' (though she didn't).

23–23. 'Me,' first use of the word. *B* was reaching for her pillow. *E* was present but at a little distance. *B* said, as if to herself, 'gi't to me, gi't to me.' Then, as if pleased with the sound of the word, she said several times 'me, me, me.' As *E* says, she probably hears the children use this phrase and stores it up as a whole. 'Give it to me' is associated in her mind with appropriative activities observed in others.

Although *B* is apparently beginning to use 'me,' 'I' and 'my,' she does not by any means employ them wherever they would be in place. Thus *E* says, 'Ask Rutger to put you up.' She goes and says 'Ask Yacky put up,' a mere repetition.

Just now *B* suddenly began tugging at a piece of tape attached to some work *E* was doing, crying emphatically, 'My, my.' She did this for a few seconds and then turned to something else. She never [?] used the word before except by mere echo.

23–25. Said 'Baby's shoe' without suggestion. First [possessive] use of this kind of name for herself.

[The reader will note that the use of the subjective names for the self — 'I,' 'me' and 'my' — begins almost simultaneously with objective names like 'baby.']

23–26. To-day *B* was riding in her cart and put her hand on the wheel. [She had been scolded for doing this.] She exclaimed 'Why, baby' in a tone of reproof, just as she says 'Why, Yacky' to *R*.

23–28. [By her mother.] Playing with silver cup. I said 'Who is in there?' seeing the reflection. She looked in carefully and said 'Mama' (I was looking in too), 'baby.' I said 'See the blue dress.' She put her hand on her own dress. Presently she put her hand on her head (not looking in the cup) and said with a queer little look, 'My baby.' As when a familiar idea looms up in a new light, not a smile.

23–30. *B* is learning to talk very rapidly. She will look at a picture of two girls and say 'Lilly girl walk-walk. Other lilly girl walk-walk.' When she falls down she says 'You'll fall,' and then 'Hop up.' [Echoes, of course, of what has been said to her on such occasions.]

This afternoon as *B* was lying on the bed she said, *apropos* of nothing, 'This is me, this is me.' Apparently there could be no mistake about the words, but I am not sure what she meant.

23–31. She was interested in her shadow on the wall this morning, putting her finger on it and saying, 'Baby, baby.' She has known this for months and got it directly, as the name of the shadow.

She seldom if ever calls herself 'baby.' [The cases previously recorded remained for a time almost unique.]

24–3. *B* still refuses to name her reflection in the glass — usually; though occasionally she will say 'Baby.' She seems a very little conscious about it now.

'Where's my comb?' *B* said this many times yesterday — imitatively. It began in her taking *E's* comb, who said 'Where's my comb?'

24–4. Sitting on the piano this evening *B* said two or three

times, with emphasis, *apropos* of nothing obvious, 'Dis my.' Don't know just what she meant.

24–6. *B* was playing with her pillow up stairs and talking to herself. She said 'I carry pillow' several times = taking it from the box to the chair, etc.

E dropped the book and said 'There goes our book.' *B* said 'There goes *my-y* (emphatic) book.'

When *B* wants to be carried she says 'I carry,' an echo of 'Shall I carry you?,' or she says 'Takey you, takey you,' echoing 'Shall I take you?' These are words, total symbols: she does not analyze them.

24–7. Just now she was sitting on my lap. She reached toward *E*. 'What do you want?' 'Shee-e you' = apparently 'Come and see you.' [It was still doubtful whether she had acquired the proper use of 'you.']

B occasionally repeats sentences beginning with 'I' that she has heard; *e. g.*, 'I guess it's so.'

24–8. *B* says 'I'll carry you' when she wants to be carried; an echo of course. 'Yacky tease you' (me). [On the other hand 'you' is sometimes used correctly.] *B*, crying, says 'I want you,' apparently her mother.

24–9. At breakfast *B* says 'Come see me' to the stuffed duckling. She probably learns such phrases as wholes = 'I will take it' or the like.

M said 'I wish baby could go out doors.' For some minutes thereafter she [*B*] said at intervals 'I wish out doors.'

24–10. She constantly repeats [after someone else] phrases containing 'my,' *e. g.*, 'Don't touch my (sewing-machine) wheel.' She seems to put more emphasis on 'my,' dwelling on it as if it impressed her.

B shows great aptitude for exclamations — doubtless because they are uttered with great emphasis. She has recently used 'Deuce,' 'Gee Whiz,' 'Oh My,' 'Oh Dear,' 'Why Yacky.' N. B. 'My' and 'I' are perhaps learned on nearly the same principle, *i. e.*, an expression of vivid feeling.

February 19, when Leila went away, *B* could scarcely if at all put two words together. She had a few single words besides our names; *e. g.*, 'street-car.' Now, not two months

later, she uses words quite freely and gets off such sentences as this (probably copied, but not immediately) 'Where my nut, squirly says' (no attempt to give exact sound).

24–12. She just said 'Where's my ball?' (We had been playing with it.)

She says 'Take you,' 'I carry you,' meaning, apparently, 'You take me,' etc.

She just said 'Eat my spoon.' (She had a spoon in her mouth.) Can't tell how she understood it.

The use of 'you' for 'I' is very frequent.

24–13. *B*, looking for *E*, said 'Hello, baby' [echoing what her mother might say when found].

24–15. She says 'I take you' (You take me), 'Come and see me (you) and many similar phrases. But she perhaps does not identify 'me' with anyone else. They are mere [imitative] formulæ describing action. 'I carry you, shovely,' she says to the shovel. To the candle 'I takey you.' [These last are examples of how 'I' may be correctly used in describing action before its special meaning is discriminated.]

She dropped the harp and wanting *R* to get it, said 'Want that, Yacky?' He did not move: then she got down, saying, 'I get it for you.' This last phrase is one she hears her mother use, and probably means for her merely 'pick it up' or the like — perhaps with the idea of self-action.

To-day she said 'Baby wash hands,' meaning she wanted to wash. She does this [uses 'baby' to name herself] occasionally now, not often, but apparently more often than she did.

'Want you' = 'I don't want you.' N. B. 'You' here seems used correctly.

24–16. She just called out as she was going up stairs, 'Going to find my dolly.'

But this [use of 'my'] is rare. Did she mean possibly 'Margaret's'?

24–18. She caressed *E* just now and said softly and deliberately, 'This my mama.' And immediately put her finger to her face and said 'baby.' It looked like an effort of self-social consciousness. This is rare if not unique.

24–20. *B* has occasionally used 'I,' 'my,' 'me' and 'baby'

apparently as names for herself (as previously noted) but at this date she uses none of them commonly. When she wants a thing she simply names it without personal pronouns. *She has no name for herself in regular use.* Though she names the rest of us constantly.

B's imagination is developing. We have told her the wolf and piggy story, and she thinks about it. She came up to *E* yesterday and said 'Wolf say puff! puff!' To-day she seemed to think the wolf might be down the register, saying, 'Wolf down there? No, no, wolf.'

At dinner to-night she asked for potato by saying 'Baby some tato' but did not use this form again. She also said 'I takey pillow' when she picked it up to bring to me. This evening again, 'Baby lie down.'

24–21. She seems to have begun the free use of 'baby' as a name for herself. The first thing this morning she said 'Baby want matches' and 'Baby's shoes.'

To-day she picked up a handful of grass and said, softly as if to herself, 'I gotty grass.' Then *E* picked up a handful and she said 'You gotty grass.'

She now uses both the pronominal and 'baby' form. I am curious to see which will prevail.

[Between the last and the succeeding.] This morning *B* said 'Baby's leg.' Immediately after she said 'I sit down.'

24–24. My for me. 'My some tato.'

24–25. 'I' and 'baby' together. 'Baby go getty hook. I gotty hook. Gi' it to mama.' She seemed to be dramatizing, speaking *to* baby. She uses 'baby' a good deal, 'I' rarely.

You. 'You takee book too,' meaning herself.

24–26. My. 'Takee my hand and pull.'

25–2. Looking for a picture of a baby in the magazine she said 'Baby, where are you?' Probably imitative? [The correct use of 'you' was still uncertain. 'You' mostly meant herself.]

B uses 'baby' now a good deal. Also, occasionally, 'I' as just now 'I take the rooster.' She still uses phrases like 'I take you' (= You take me).

'I' used rightly a good deal: 'I get this ball.' Have not

noticed 'my' recently. *E* says she occasionally says 'Take my hand.'

25–4. Although she uses 'I' correctly at times she almost always (quite?) uses phrases she might take bodily from others — as 'I'll get the carriage,' etc. I incline to think she learns these as wholes.

B wanted the knife. *E* said emphatically, 'My knife.' About fifteen minutes later *B* said 'Want that *my* knife.' She took it and repeated 'My knife.' [This is perhaps suggestive as to how 'my' becomes associated with the expression and act of claim.]

Mixing 'I' and 'you' (as self-names). 'Mama carry you (me). I go play sand-pile.' Also to her doll-carriage, 'Stay there. I'll go pick stones.'

25–5. She uses 'I' in sentences of action rather frequently — perhaps a dozen times a day, *e. g.*, 'I getty cushion.'

25–6. 'Have that my knife. *My* knife.' [See 25–4.] Immediately after she said 'Papa help you' (me). She doesn't use 'me' in its right sense, if at all.

25–7. 'Baby, I.' Sometimes she unites them: 'Baby I got two, mama.' She uses either quite freely in this sense.

Long sentence. 'When Yah-Yah ring bell ding-a-ling-a-ling, then come dinner.'

25–8. 'Where's the pillow?' 'I'll get it for you.'

25–13. She says 'I got two flowers.' Is very fond of 'I got two —.' Last evening she was saying 'My Margy' and 'My papa.' I never heard her use 'my' in altercation. [This refers to a theory I had that the first personal pronouns, especially 'my' might be learned in disputes over possession.]

25–14. She uses 'baby' now a good deal. ['I,' 'my' and 'me' rarely.] Will these be lost?

25–16. *E* thinks that *B* frequently says 'Come see me,' etc. It is often hard to tell whether she says 'me' or 'baby,' she has come to slur the latter so.

'Margy, come see me baby.'

25–21. *M* said, 'I want some bread.' *B*, '*I* want bread.' [The stress seems to show a sense of the self-assertive meaning of 'I.']

25–21. The use of 'I' seems to be gaining rapidly on 'baby.' The former is now, perhaps, the more common.

[Additional examples of the correct use of 'I' and 'me' are omitted as superfluous.]

[Between the last and the succeeding.] 'Mine,' first use. She was talking to herself, making-believe finding things and holding them out. 'Want necktie; here's mine; here's mama's.'

26–1. 'I won't.' Said reflectively, not controversially, this morning when asked to come up stairs. [An echo from other children?]

26–2. I-phrases. She uses many which she seems to learn as wholes — 'I don't know,' 'I want —,' 'I won't,' 'Come see me,' 'Wake me up.' From these she probably gets the I-idea by elimination. [*I. e.*, the rest of the sentence varies but the pronoun remains constantly associated with the expression of will, the self-attitude.]

She has at this date no notion of calling her body 'I,' apparently.

Looking at the photograph she says 'There's baby,' never 'That is I' or 'me.' But when we ask 'Where are you?' she said, with some hesitation, 'There's baby.' (But, on further experiment, this is uncertain.) In answer to 'Where am I?,' however, she named [pointed out in the picture] the speaker.

26–4. Taking a picture from my hand she said 'That's mine.'

26–6. My. Although she sometimes uses it correctly she also uses it incorrectly. *E. g.*, 'Mama go get my [your] lap-board.' 'Take my [your] leg up.'

She never, I think, says 'your,' 'her' or 'his.' (Except your = my, by direct imitation.)

She uses 'me' with more emphasis now.

She said twice to-day 'That's mine.'

26–11. She still says 'I carry you,' much run together, for 'take me up.'

Not only does she still say 'I carry you' for 'You carry me,' but in many other phrases as 'You want cake.' Often she will use 'You,' 'I' and 'baby' in quick succession to mean herself.

26–13. Quickness of children in applying language. It is

amazing, hardly to be fully realized or explained by adults. They are constantly bringing out very aptly some expression they have heard only a few times. Thus to-day *B* said to *R* 'Ring off' when he was teasing her. [Current slang for 'cease.']

26–16. *R* was threatening to take away a necklace. She almost screamed 'Dat mine.' Speaking of her dress she said 'That's *my* dress; that's *mine;* that's *baby's*' all in the same sentence. [The gradual increase of appropriative emphasis on the self-words is notable.]

26–17. Saying 'My book' with great emphasis, at times almost a scream, when threatened with dispossession by *R*. [Compare 25–13.]

26–18. Emphatic 'you.' '*You* go get the ball.'

26–21. 'Herself.' 'Baby hurt herself' she said to-day when she fell down.

26–22. 'She' for self. Is common; *e. g.*, 'She does not want to go to bed.' Imitative of some one's remark about her. [This form became very conspicuous about this time.]

26–24. 'She.' To-day she has used this more than 'I' or 'baby.'

26–28. *E*, 'This is mama' (pointing to *B*); 'This is baby' (pointing to herself). *B*, 'No, *mine*, baby.' [This seems to be the first definite application of a self-pronoun to the physical person.]

27–2. Although she uses 'I,' 'me,' 'my' and 'mine' freely and vigorously she still uses such phrases as 'Mama wants to carry you.' (= I want mama to carry me.)

'We don't want papa,' she said to-day. [First recorded use of 'We.']

27–8. 'I don't want to bite you.' (= You don't want to bite me.)

She uses right and wrong expressions with great freedom and indifference. 'She,' 'baby,' 'I,' 'you,' may each mean herself.

27–9. 'I want to take a walk with me' (you).

27–14. She uses 'she' for 'I' a good deal; but 'I' equally or more. Also 'baby.' She is fond of saying 'I think' or

'She thinks.' Sometimes she uses this form, 'She says she wants two dollies.'

27–25. *B* still uses 'I,' 'baby' and 'she' interchangeably. She is beginning to say 'Mary Liffit' [Mary Elizabeth] also.

28–8. She varies for emphasis. 'I like cake! She likes cake! Baby likes cake!' in quick succession.

'That's yours, Margaret, and that's me's.'

28–27. 'I,' 'baby,' 'she.' *B* still uses all three for herself. I think she tends to use 'baby' rather more for 'me' than for 'I.' 'She' is perhaps somewhat diminishing.

29–26. I notice only an occasional use of 'she' and 'baby' now; also that 'I' and 'mine' are used with more mastery and gusto.

30–2. Heard her use 'she' [for I] but it is rare.

30–12. *E* says that *B* still uses 'she' and 'baby' a good deal, though less than she used to.

30–27. *B* still uses 'she' quite often, though 'I' and 'me' are much more usual. Sometimes she gets queer combinations — as 'me are.'

31–1. She just now said 'Baby going to dance 'se'f' [herself].

31–9. B has learned to know *T*. To-day she accidentally made one with her blocks. 'Come see my *T*' she exclaimed. Then she made many more, quite excited, crying '*I* did it,' etc.

32–26. Some peculiarities in pronouns. "Yes we am — Huh! 'we am' that's a funny way to talk" [self-criticism]. 'Let I see it.' 'She's name is Binks.' I and she are often used in the objective case.

33–5. We note that *B* no longer uses 'she' for 'I.' She has gradually dropped it.[1]

[1] The MS. of this article was received on August 1, 1908. — ED.

THE NERVOUS CORRELATE OF ATTENTION.

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INTRODUCTION.

In a previous article in this REVIEW[1] I developed a theory of the relation between nervous function and consciousness. However, I had to be very brief in the treatment of some aspects of this relation. It was then my chief intention to combat that mythological theory of pleasantness-unpleasantness, according to which Pleasantness and Unpleasantness are two ghost-like entities sitting within our brain — armed, one with shears, the other with a soldering iron (according to a variant: with a stamping iron) — breaking and making connections at will. It is my conviction that, just as it is possible to comprehend the reciprocation of the action of a steam engine as being caused by the engine itself, so it is possible to comprehend the modification to which the instinctive action of a higher animal is subject, as being governed by the fundamental laws of the nervous system itself.

I shall now treat in more detail a number of questions concerning the relation of nervous function and consciousness which I could not take up previously without leading the reader's attention too far away from the problem of pleasantness-unpleasantness. But we shall no more restrict ourselves now to an exclusive discussion of the non-affective consciousness than we

[1] 'The Nervous Correlate of Pleasantness and Unpleasantness,' PSYCHOLOGICAL REVIEW, XV., 4, 5, 201–216, 292–322, 1908.

restricted ourselves then to an exclusive discussion of pleasantness-unpleasantness. It is, of course, only by abstraction that we divide our consciousness into classes to be kept in different compartments. Every application to actual life forces us to take into consideration the mutual relations of these classes, although our scientific interest may momentarily seem to be confined to one of them.

Meanwhile Titchener's *Psychology of Feeling and Attention* has been made public. If there is anyone whom the historical part of my previous article did not convince that the traditional methods of investigating feeling — no matter whether experimental or otherwise — have reached a *cul-de-sac*, Titchener's book will convince him. The author's conclusion is decidedly pessimistic, one might even say desperate. He confesses "to a feeling of unpleasantness, tension and depression. We know so very little of the subject of these Lectures, and the work that we have found to do will take so long in the doing!" This will hardly encourage any student to devote himself to an investigation of feeling. What we need is a new point of view. I offer my own view with the hope that it will give the reader as much relief, encouragement, and pleasantness as it has given me.

I. The Fundamental Laws of Nervous Function.

Let me begin with an abbreviated statement of the laws of nervous function as found on pages 300 ff. of my previous article.

1. Let us assume that any point where two or more neurons are connected, functions like a valve which permits fluid to pass through only in one, not in the opposite direction, and that this valve opens only in the direction towards motor points of the body.

2. Let us assume that any neuron system has functional properties analogous to those of a pipe system filled with a fluid, one end of the pipe system being usually closed (the sensory point), the other end being under the influence of negative pressure, or suction, tending to produce a current in the direction of this end, the motor point.

3. Let us assume that ordinarily no movement can take place in the pipe system because the sensory end is closed; that the application of a weaker or stronger stimulus means the same as the opening to a lesser or greater extent of the closed end of such a pipe system, thus permitting fluid to escape through the motor point (or points).

4. Let us assume that the resistance suffered by the fluid in moving through the pipe system depends in the ordinary way on the length and the width of the pipe.

5. Let us assume that the pipe is widened by the current in proportion to its intensity and duration, and that, while unused, the pipe has a tendency to grow narrower; and that this *susceptibility* of the pipe is the more pronounced the 'higher' the connecting neuron.

To these assumptions was added on page 303 a sixth one, the most important one in so far as it serves to explain the function which is analogous to that of a steam-engine's *valve-gear* — a function for which thus far mythological explanations have been offered rather than scientific ones.

6. Nervous currents are subject to a law analogous to the dynamic law that hydraulic pressure is less than hydrostatic pressure, of which the jet pump is a familiar illustration.

For the reason already mentioned I did not attempt in my previous article to justify these assumptions except by applying them to habit formation and to the rôle played by pleasantness-unpleasantness in mental life. I shall now give additional reasons why it seems to me to be wise to make these assumptions rather than others.

A nervous process is generally regarded as a chemical process. But of what kind? It is most frequently compared with the burning of a fuse. In my opinion there is no analogy in this case at all. But before giving reasons why I regard the analogies offered by myself as scientifically valuable, let me say a word in reply to the possible question, why we should use analogies rather than wait until other sciences are able to tell us exactly what the nervous process is.

I take it for granted that a teacher of psychology wants to give his students more than a mere classification of mental

states, that he wants to show them how the laws of mental life can be understood as a part of the natural laws governing a highly developed organic being. What does he answer to the student's question in what manner the nervous system is modified when instincts are modified into habits? Unless he can offer to the student a *picture* — however hypothetical — of the laws of nervous activity, he is obliged to offer mythological entities making and breaking connections at will. I am far from dogmatically asserting that it is inconceivable that pleasantness and unpleasantness make and break connections in the brain; but this is an entirely empty, purely formal statement, as long as *we are not told the exact nervous or mental conditions under which pleasantness and unpleasantness themselves occur*. We cannot explain to the student how our actions are determined by our mental life without a *detailed picture* of nervous activities. Since experimental physiology at present does not offer us such a picture, at least not one which serves the purposes of the psychologist, we have to develop one ourselves. We cannot wait for physiological discoveries which may never be made.

So much in excuse of my making assumptions. Let us return to the statement that we cannot picture a nervous process as the burning of a fuse. A fuse burns in either direction. A nervous process, it is true, if started in a neuron, is propagated in both directions within that neuron. But it does not seem probable, so far as I can judge the experimental evidence, that a nervous process can normally pass from one neuron into another neuron which is *nearer* to a *sensory* point of the body. If this were otherwise, it would be possible for central excitations to produce normal sensory processes in our sense organs. This seems improbable, in spite of so-called synesthesias, which in my opinion must be otherwise explained. I have therefore accepted the first assumption that any point of connection functions like a valve which permits fluid to pass only in one, not in the opposite direction.

Another reason why it seems confusing to picture the nervous process as the burning of a fuse in this: If a fuse branches out, the burning would proceed along all the branches, regard-

less of differences in the *length* of the branches. But the nervous process does not branch out in this manner. However important the law of nervous diffusion may be, this kind of diffusion — analogous to the burning of a branching fuse — does not exist. Another point of disagreement is the following: A fuse cannot be made to continue burning for a considerable time by mere persistence of the cause of the burning. But a nervous process can be made to continue by continued stimulation, although not for an indefinite time.

It seems to me most probable that the nervous process is of the same nature as the migration of ions in an electrolyte. The migration of ions is accompanied by electrical phenomena which permit a diagnosis of the migration and a measurement of the flux of ions. Similarly the nervous process is accompanied by electrical phenomena. Migration of ions can be caused by the electric current: so can the nervous process. But migration of ions is also dependent on and caused by many other physical and chemical conditions: so is the nervous process. In spite of these and other agreements, I did not in my second assumption picture the nervous process as a migration of ions for the following reason. Some one who might think that he knows that the nervous process is not a migration of ions, might forget that I offered merely a picture and might argue against the whole theory. No one, surely, will forget that speaking of the nervous system as a pipe line is merely using a picture, an analogon, so that his refusal to admit that it actually is a pipe line, cannot be made an argument against the theory here offered.

A possible confusion of terms may be anticipated and prevented by explanation. When we think of a fluid, first at rest and under a constant tension ('negative pressure'), then moving, the word 'velocity' may be used in two different meanings, namely, as standing for the flux or for the velocity of the propagation of the relief of tension. In a pipe closed at one end, and with suction applied to the other end, the whole column of fluid is under tension. As soon as the closed end is opened, the tension is relieved at this end, and the flux may begin. But at any distant point the flux can begin only after the relief of tension

has occurred at that point; and the whole column of fluid can be in motion only after the relief of tension has traveled all through the pipe and reached the open end. The velocity with which this relief of tension travels is analogous to the velocity of the propagation of the nervous process as usually measured and found to be about 60 m. per second or less. It is *independent* of the size of the opening at the sensory point, that is, of the intensity of stimulation. On the other hand, the term velocity might be used in the sense of 'number of fluid units which pass a cross-section of the pipe in one second.' Velocity in the former sense does not concern us in our present discussion. In the latter sense it is an important concept; but owing to the equivocality of the term, I shall avoid it in the future and use the word flux. Flux, then, is analogous to intensity of the nervous process.

The assumption of a constant suction at the motor end of the pipe line is merely analogous to the fact that the nervous system, unless exhausted, is always ready to respond to stimulation. It does not, of course, imply that the *muscle fibers* act like an exhaust pump. I do not conceive either of the sensory or of the motor points of the nervous system as points where energy is transmitted, but as points where the performance of work is made possible by the fulfilment of a necessary condition. Sensory stimulation may be compared with the immersing of the zinc in a plunge-battery — the resulting migration of ions is of course not the equivalent of the mechanical energy of the motion of the zinc, although the greater the immersing movement of the zinc, the greater the flux of ions. Innervation of the muscle may be compared with a signal for action given to a body of men by means of an electric bell. Again there is no energy equivalent. This will explain the third assumption.

The fourth assumption needs no explanation. The fifth assumption shows clearly the advantage of using a mechanical picture as analogon of the nervous process. We can easily conceive a pipe line, the inner wall of which consists of a material that may be washed out by action of the fluid. Just recall a limestone cave. And it is also easy to think of a deposit on the walls resulting from lack of use. Let us call this property

of the pipe, enabling its resistance to decrease or increase, the *susceptibility* of the pipe. This susceptibility I assumed to be the greater the higher the connecting neuron in question. I did this for the following reasons: (1) Our sense organs do not seem to respond more promptly to stimuli to which they have often been exposed. Rather the opposite. Blind-born persons who for years have made little use of their eyes, have after operation no inferior, but rather a superior visual sensitivity. It seems best, then, to assume that the sensory (and also the motor) neurons have no susceptibility as above defined, but that this belongs only to the connecting neurons. (2) It is generally accepted that the higher nerve centers suffer more easily from pathological influences than the lower ones. I have therefore accepted the assumption that the higher connecting neurons have a greater normal susceptibility than the lower ones.

The sixth assumption shows again the advantage of using a mechanical picture of the nervous process. Everybody is familiar with the principle underlying the function of a jet pump. No vague conception, but a clear image of a familiar instrument guides our thought. What is possible in a mechanical system — the attraction of a weaker process by a stronger one — must also be possible under physiological laws, which are far more complicated than those of mechanics. Of course, there is this difference between the mechanical instrument in our theory and the ordinary jet pump, that in the latter the moving force is pressure, in our case it is suction, which requires a somewhat different mechanical arrangement.

The second and sixth assumptions would not be possible without the first. If the nervous process could pass in either direction through any connecting point of neurons, flux might easily occur *from one motor point* in the direction of another, as soon as the force of suction should fail to be exactly alike at all motor points. And the change from hydrostatic to hydraulic pressure would also cause, by attraction, flux *from motor points* to a motor point. This seems to lead to impossible complications, and is an additional reason for making the first assumption.

II. The Uniqueness of Pain Reactions.

Instinctive reactions upon pain show a remarkable difference from other instincts. Unless this difference is clearly understood, pain reactions present a serious difficulty to our theory of nervous function. We have assumed that a stronger nervous process attracts a weaker one. A strong and continuous pain process, for example one originating from a sore tooth, might then be expected to attract all other nervous processes from whatever sense organs they originate. Since the pain process is there first, the new nervous processes, it seems, can never attain constancy of flux, but as soon as they originate must join the pain process. The flux of the latter, therefore, is again and again increased at points other than sensory points of the body. Must we not conclude then from our theory that a person who has a strong toothache should uninterruptedly be engaged in the same muscular activity, and that a toothache should be a highly pleasant experience?

To some extent this difficulty can be met theoretically by the following explanation. Touching and pressing upon the tooth does probably weaken the sensation of pain somewhat. But as soon as our hand has now begun to respond to some other stimuli acting upon our eyes, ears, or other sense organs, the pain stimulus starts again with renewed vigor, and the renewed process coming from the pain point deflects these other nervous processes soon after they have become constant, with resulting unpleasantness.

Of much more importance, however, is the following fact, which makes it plain why a strong sensation of pain must be unpleasant although the stimulation may continue strong. If the instinct of, say, swallowing is properly stimulated, the subject swallows. If the stimulus continues, the subject does not scratch himself behind the ear or spit, but swallows again and again. But the instinctive response to a pain stimulus is not a repetition of the first reaction. If I perceive pain at a definite point of my body, I perhaps move first my finger toward that point. If in spite of this movement the pain should continue, could it be of much value to me to repeat the movement? This movement obviously failed to remove the cause of the sen-

sation of pain. *Another kind* of movement must therefore occur, as a biological necessity, — and actually does occur, instinctively. If the pain still continues, a *third kind* of movement occurs; and so on. We may describe the difference between the instinctive pain reaction and other instincts thus: If the pain reaction is repeated because of a continuance of the stimulus, this does not mean that the same movement is repeated, but a different movement occurs each time. The nervous process, although starting from the same sensory point, does not, as in other instincts, continue to flow over the same path in the same motor direction, but suddenly takes a different path, after a while, if the stimulation continues, again a different path, and so on. Everybody knows what dancing and writhing movements result from continued pain, wherever the stimulus may be located. By what mechanism this recurrent change of action is brought about, I am unable to suggest. But the fact of its being brought about by some structural or functional peculiarity of our pain nerves cannot be doubted.

If we apply this to the above case of a sore tooth, we understand why a continued toothache cannot be pleasant. We at first thought of the nervous process resulting from the stimulation as a process going on continuously over the same path, and forcing all later arising and weaker processes to join it. We now see that the pain process is not one long-continued process, but many different processes succeeding each other. Every time another pain reaction occurs, the new nervous process interferes with the other processes going on at that moment, coming from various sense organs, by deflecting them. Unpleasantness must therefore be felt.

What has been said of pain reactions applies largely also to the instinctive reactions upon those other sense impressions which are harmful to the body, as bitterness or acidity. Having got a bitter substance into our mouth, we at first simply spit; but if the bitter sensation continues with any strength, we instinctively do many other things, wash our mouth with our tongue and with our fingers, put other things in our mouth, drink water, etc. For the same reason, therefore, an uninterrupted stimulus of bitter or sour is not likely to produce a sensation accompanied by pleasantness.

III. The Susceptibility of Connecting Neurons.

That property of the connecting neurons which in our fifth assumption we called susceptibility, is of much significance in several respects. In my previous paper I did not make any statement concerning a definite ratio of the susceptibility of higher and lower neurons, but merely stated that the susceptibility of the higher neurons was greater. The assumption of definite values, however, leads to important consequences.

It is necessary that we distinguish between the two kinds of susceptibility by giving them definite names. Let us call *positive susceptibility* that property of a neuron which consists in a decrease of resistance in consequence of carrying a current, *negative susceptibility* that property which consists in an increase of resistance in consequence of lack of function.

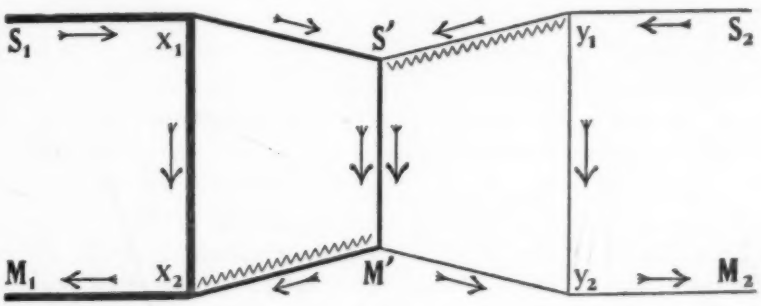

Fig. 1.

Let us arbitrarily assume that the positive susceptibility of each of the three neurons x_1S', $S'M'$ and $M'x_2$ in Fig. 1 is three times that of the neuron x_1x_2; and further, that at the earliest time when all these neurons have reached their complete development, the resistance of x_1x_2 is equal to that of each of the neurons x_1x', $S'M'$ and $M'x_2$. Suppose now that the point S' is stimulated. The flux is distributed according to the resistances offered by the paths. The resistance of the path $x_1S'M'x_2$ is three times the resistance of the path x_1x_2. The flux at any point of the path x_1x_2 must therefore be three times the flux at any point of the path $x_1S'M'x_2$. The resistance of both paths must decrease because of the current in both. If the decrease

depended on the flux alone, it would be three times as great in x_1x_2 as in $x_1S'M'x_2$. But we assumed the positive susceptibility of the latter path to be three times that of the former. The decrease of the resistance is therefore exactly the same at any point of $x_1S'M'x_2$ as at any point of x_1x_2. Consequently, when the stimulation at S_1 ceases, although the resistance of all the four neurons in question has decreased, we find again that the resistance of x_1x_2 is equal to the resistance of each of the neurons x_1S', $S'M'$ and $M'x_2$. In this consideration, for simplicity's sake the existence of the path $M'y_2$ has been neglected. The flux from S' to M' must of course divide at the point M'. But most of it must take its way toward x_2, much less towards y_2, because of the attraction by the current going on in $x_1x_2M_1$.

The negative susceptibility has not yet become effective. Now, how will the relative resistance of the four neurons x_1x_2, x_1S', $S'M'$ and $M'x_2$ be found after some time? If the negative susceptibility does not differ in these neurons, no change will occur. If it is greater in each of the three neurons x_1S', $S'M'$ and $M'x_2$ than in x_1x_2, the resistance of the path $x_1S'M'x_2$ would soon become greater than that of x_1x_2. Is this probable? To some extent it seems to be true, for instincts cannot be trained, that is, modified, so well at a time much later than their natural appearance. "Was Hänschen nicht lernt, lernt Hans nimmermehr!" But special experiment alone can give an answer to the question as to just how much greater the negative susceptibility of higher neurons is than that of lower ones.

With respect to the positive susceptibility, if we had not made the assumption that in each of the neurons x_1S', $S'M'$ and $M'x_2$, it was three times that of x_1x_2, but the assumption that it was less, say only twice as great, what would have been the consequence? Obviously this: At the time when the stimulation at S_1 ceased, the resistance of the neuron x_1x_2 would have been found — not equal to, but only two thirds of the resistance of each of the neurons x_1S', $S'M'$ and $M'x_2$. Repeated stimulation would then result in a pronounced widening of the channel x_1x_2 and a relative or absolute (because of the negative susceptibility) narrowing of the channel $x_1S'M'x_2$. This is the case spoken of very briefly on page 302 of my previous

paper as a habit which is merely a strengthened instinct — strengthened in such a way that *any later modification becomes impossible*.

If the ratios of the positive and of the negative susceptibility are assumed to be exceedingly different, a habit can be conceived which is merely a strengthened instinct, but which *can easily be modified at any time*. Let us assume that the positive susceptibility of each of the neurons x_1S', $S'M'$ and $M'x_2$ is ten times that of x_1x_2; and that the negative susceptibility of the former is but a trifle larger than that of the latter. Now suppose that the point S_1 is very frequently stimulated, each time for a considerable length of time, and with but short pauses between the stimulations. It is clear that then — in spite of the flux being at first at any point of x_1x_2 three times as great as the flux at any point of the path $x_1S'M'x_2$ — the resistance of the path $x_1S'M'x_2$ will finally be only a very small fraction of the resistance of the path x_1x_2, and that this ratio will not appreciably change in time since the negative susceptibility of the higher neurons is assumed to be very little greater than that of the lower neuron. We have thus changed an instinct into a habit in such a manner that the resistance of the paths leading from S_1 to M_1 is much less than it was under native conditions. In so far the case is like the one discussed above. But it is very different in this respect, that whenever now S_1 is stimulated, the nervous current takes the path exclusively over the higher center $S'M'$, whereas in the former case it would take the path exclusively over the lower center x_1x_2. The great significance of this difference for an animal's life is clear. In the former case, the reflex arch S_1M_2 has practically lost its connection, its functional communication with other reflex arches, S_2M_2, S_3M_3, etc. In the latter, the reflex arch has been brought into much closer connection with the other reflex arches, so that at any time it can enter into functional relations with them and those modifications of the response can be brought about which I have described in some detail in my previous article.

It is not, and cannot be, my task here to describe in detail what goes on in the nervous system in any definite situation occurring in a particular person's life. What I wished to show is

merely this, that under the assumption of a very few principles of nervous function, pictured in mechanical terms, the most varied adaptations of the nervous system can easily be comprehended without any necessity for introducing mythological powers of which no one can tell us whence they come and whither they go. A mere change in the quantitative relations of the properties which the neurons are assumed to possess, leads to the result that a habit, instead of being less modifiable than the instinct of which it represents a strengthened form, is on the contrary vastly more modifiable than this instinct. If we only knew more about human and animal instincts and habits and the relations between them, we could derive from this experimental knowledge the quantitative relations which we had to assume quite arbitrarily; and we could understand quite well the functions of the nervous system even if the study of the anatomy and physiology of the nervous system were doomed to remain forever on the present level of development — a prophecy which, of course, no one will make.

Let us show in another case how we can explain a special form of habit if only the quantitative relations of our principles are properly selected. On page 304 of my previous paper I applied the theory to that form of habit which I called sensory condensation. The explanation there given is open to criticism because it does not take into account the quantitative aspect of the susceptibility of the neurons. I had to be very brief at that time. I shall now discuss the same case, going a little more into detail. We supposed that both S_1 and S_2 (see Fig. 1 above) were stimulated, but S_1 more strongly than S_2. Let us suppose that the resistances of all neurons are originally the same, and that the positive susceptibility of each of the neurons x_1S', y_1S', $S'M'$, $M'x_2$ and $M'y_2$ is three times that of either x_1x_2 or y_1y_2. (If we restrict ourselves to the establishing of the habit and neglect the forgetting, we need not make any assumption concerning the negative susceptibilities.) We found above that under these assumptions the stimulation of the single point S_1, however prolonged, would not change the *relative* resistances of the neurons x_1S', $S'M'$, $M'x_2$ and x_1x_2. All these resistances, although absolutely diminished, would still be equal to each

other. But when S_2 is stimulated simultaneously with, but more weakly than, S_1, the current in $S'M'x_2$ is increased by some flux coming from S_2. This reduces the resistance of the neuron $S'M'$, and also of $M'x_2$, to less than the resistance of x_1x_2. In consequence of this reduction more of the flux coming from S_1 takes the path x_1S', less the path x_1x_2 than at first. The increased flux in x_1S' reduces the resistance of the neuron x_1S'. It is clear that the longer all this goes on the more must the path $x_1S'M'x_2$ be favored with respect to resistance change in comparison with x_1x_2. Finally the existence of the path x_1x_2 may be entirely neglected, for its resistance, while it has steadily decreased, has become very much larger than the resistance of the longer path $x_1S'M'x_2$ whose resistance has so much more rapidly decreased.

Thus far, however, the establishment of a habit of the kind which we called sensory condensation has not been explained. If S_1 alone is now stimulated, no appreciable current would occur in $M'y_2$, since the resistance of this neuron is by this time many times that of either of the paths leading from S_1 to M_1. If S_2 alone is stimulated the response would occur at M_1, not also at M_2. If S_2 is stimulated and S_1 at the same time, the response would again occur only at M_1. It is necessary, in order to bring about this special form of habit, sensory conden$_1$sation, that double stimulations of both kinds, S_1 strong with S_2 weak, and S_2 strong with S_1 weak, occur in frequent alternation from the start. Suppose the resistance of the path $x_1S'M'x_2$ has been reduced only to very little less than three times the resistance of x_1x_2, and now S_2 is stimulated strongly and S_2 weakly. Then the flux from S_2 will not take its way towards M_1, but most of it passes over y_1y_2 towards M_2 and most of the flux from S_1 takes its way over $S'M'$ towards M_2. Thus the resistance of $y_1S'M'y_2$ can by steps decrease to much less than y_1y_2. I say by steps, because this occurs while the symmetrically corresponding change on the left side of the figure, discussed above, has its pauses. The total outcome then is, that the existence of both the paths x_1x_2 and y_1y_2 may be disregarded, because the resistance of the paths $x_1S'M'x_2$ and $y_1S'M'y_2$ has become incomparably smaller than the resistance of either x_1x-

or $y_1 y_2$, although absolutely these latter resistances have decreased too. Whenever now a single stimulus is applied to either S_1 or S_2, the flux takes its path over $S'M'$ and dividing at M' produces a double response, at M_1 and M_2. This is what we called sensory condensation.

It may be well to illustrate this kind of habit. A child learning to play the piano has before him two notes to which he is expected to respond by simultaneous movement of two fingers. One note is seen by foveal vision and the corresponding finger movement occurs with definiteness. The other note is seen by extra-foveal vision and the corresponding movement lacks definiteness. The child tries again and looks now at the note whose movement was not properly executed. Everything is reversed. The finger movement which was previously done badly is now done well, and the movement which was previously done well, is done badly. After many trials, on the whole alternating as to the favorable note and finger, the child becomes able to perform both movements simultaneously with definiteness although he is looking at only one of the notes — no matter which one he is looking at.

Summarizing, then, we may say that there is no justification for the remark not infrequently heard, that psychology can make no further theoretical progress, that we have to wait until the anatomy and physiology of the nervous system have made a long step in advance. We need not be discouraged. Experimental knowledge of habits and of their development out of instincts will enable us to obtain a scientific theory of human and animal activity by permitting us to determine the quantitative values which for the present we have to assume quite arbitrarily.

(I may use this opportunity for correcting a mistake in the description of the establishment of the 'variation of response' in my previous article, p. 303. In the fifth line from the bottom, between the words actually and than, the word 'greater' ought to be substituted for 'less.')[1]

(*To be concluded.*)

[1] The MS. of this article was received September 1, 1908. — ED.

PECULIARITIES OF PERIPHERAL VISION.

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II. The Perception of Motion by the Peripheral Retina.

I. Introduction.

1. *Problem.* — In a recent paper[1] by the author, certain differences in the perception of size, between the right and left halves of the retinas, were described and their importance for a theory of the origin of right- and left-handedness was urged. In the same paper, it was mentioned that along with differences in *size*, there were also differences in the apparent *rates* of moving objects, when they appear in the periphery of the field of vision. The apparatus by which the experiments were carried out was described and figured in the paper just alluded to. In its essentials the apparatus consisted of a large perimeter, upon which two cardboard discs could be moved and clamped in any desired position. Two circular, black spots were fastened upon the discs in such a way that they could be moved towards the center of the disc, always along the same radius, whenever it was desirable to do so. The discs were mounted upon a spindle similar to that of a color mixer, which was so drilled and split as to admit the end of a flexible shaft. The other end of the shaft was introduced into the axle of a pulley and the pulley itself was turned by an electro-motor. As both discs were connected in the same way with the same pulley which was driven by a motor, they turned necessarily at the same rate, although the directions of motion of the discs were opposite. It is evident that these conditions afford the best opportunity for comparing the size, rate of motion or form of objects in the periphery of the field of vision. Our problem, then, is to compare the rate of motion of one retinal locality with that of another (only one eye being used at a time), in order to determine the manner and

[1] Psych. Rev., 15, 69.

degree in which the apparent rate changes; and also to determine, if possible, the cause of the apparent differences in rate. As a part of the paper is to be critical, a general statement, at the outset, of the conclusions may serve to keep the issues clear. (1) When two moving objects which have the same rate, extent and form, of motion appear in different parts of the periphery of the field of vision, the object in the right half, appears to move faster than a similar moving object in the left half, or at the center, of the field of vision. (2) This result is the same for both eyes. (3) The distribution of the illusory differences in rate of motion, within the visual field correspond exactly to the distribution of the illusory differences in size which were reported in the paper already referred to and of which two examples are given in this paper (Figs. 1 and 2). (4) The cause of the illusory motion is the difference in space perception in different localities of the retina.

2. *The Experiments of Others.* — That a moving object appears to move considerably faster when seen in indirect vision than when seen directly has come to be generally accepted. Exner[1] first reported the fact in the paper which is so brief that it is reproduced in full below. Muensterberg in the Milton Bradley Pseudoptics, gives a simple device by which the observation may be made. Dresslar[2] also describes an experiment by which the fact may be demonstrated. In a recent paper[3] by R. Cords and E. Th. von Bruecke, on the velocity of afterimages of moving objects, it was found that "Unter sonst gleichen Versuchsbedingungen ist die Geschwindigkeit des Bewegungsbildes in den peripheren Anteilen des Gesichtsfeldes bedeutend groesser als in den zentralen." Basler[4] found that the overestimation of the motion was not confined to the periphery of the visual field, but occurred in the central region as well.

To this consensus of opinion, there is, so far as I know, but one dissentient voice. Sanford[5] attributes the difference to the movement of the eye in direct and to its non-movement in in-

[1] *Pflueger's Arch.*, 38, 217.
[2] *Am. Jour. Psych.*, 6, 312.
[3] *Pflueger's Arch.*, 119, 76.
[4] *Pflueger's Arch.*, 115, 582, 'Ueber das Sehen von Bewegungen.'
[5] Sanford, *Experimental Psychology*, 306.

direct, vision. "The statement is sometimes made that the apparent rate of moving objects seen indirectly is greater than that of the same objects seen directly, and certain experiments seem to give ground for the statement. It is probable, however, that this is an error, and that an important factor has been omitted in the interpretation of the experiments; namely, that in direct vision the eye follows the moving object, and in indirect it does not. When this difference is avoided, it is hard to perceive a difference in rate in the two conditions."

The empirical evidence for the fact rests npon three experiments: Exner's with a candle on a pendulum, Dresslar's with a pendulum reflected in a mirror and Muensterberg's with a rod which is held in the hand and turned back and forth through a small arc. Exner's experiment and his results are sufficiently explained in the quotation which follows.

Ein Versuch über die Netzhautperipherie als Organ zur Wahrnehmung von Bewegungen.

Vor Jahren habe ich eine Reihe von Beobachtungen publicirt,[1] welche zeigten, dass die Netzhautperipherie, bei ihrer bekannten Unvollkommenheit in der Wahrnehmung raumlicher Formen, im Vergleich zum Netzhautcentrum, doch in hohem Grade befähigt ist, Bewegungen (und wohl Veränderungen überhaupt) zu erkennen. Sie spielt die Rolle eines Wächters, welcher die Aufgabe hat jede verdächtige Änderung in seinem Gebiete zur Kenntniss zu bringen. Ist eine solche einmal bemerkt, dann wird instinctiv der Blick dahin gerichtet und das Vorkommniss mit der Macula lutea genauer geprüft. Mein College v. Fleischl[2] hat eine Hypothese über die Retinastructur aufgestellt, welche diese Bewegungsempfindlichkeit der Netzhautperipherie zu erklären geeignet ist. Im Folgenden theile ich einen Versuch mit, der diese Hyperasthesie für Bewegungen einem grösseren Publicum zu zeigen gestattet. An einem Pendel von 1–2 Secunden Schwingungsdauer ist eine brennende Kerze angebracht. Man lässt, während das Pendel in Ruhe

[1] 'Das Sehen von Bewegungen und die Theorie des zuzammengesetzten Auges,' *Wiener Akad. Sitzber.*, 1875, Bd. 72.

[2] 'Physiologisch-optische Notizen, III.,' *Wiener Akad. Sitzber.*, Bd. 87, Mai, 1883.

ist, einen Punkt fixieren, der für den einige Meter entfernten Beobachter die Kerzenflamme unter einem Gesichtswinkel von c. 45° erscheinen lässt, und weist ihn an, die Elongationen der Flamme zu beachten, um ein Urtheil über die Grösse derselben zu fällen. Dann erst setzt man das Pendel in Bewegung, am besten so, dass die Elongation unter 10 cm. beträgt. Lässt man sich nun die geschätzte Elongation entweder mündlich in Centimetern oder durch Handstellung angeben, so pflegt dieselbe 2–3 mal zu gross auszufallen. Ich habe nie gesehen, dass jemand die Elongation unterschätzt hatte. In der beistehenden Tabelle sind die Schätzungen enthalten, welche Besucher des Laboratoriums in Laufe einiger Tage auf meine Bitte ausgeführt haben, und zwar ohne Auswahl. Sie stammen von Herren her, welche nicht wussten, um was es sich bei dem Versuche handelt, die also nur den Auftrag hatten, die wahrgenommene Elongation schätzungsweise anzugeben.

Versuchperson.	Wirkliche Elongation.	Geschatzte Elongation.
Dr. P.	10 cm.	25–30 cm.
Dr. K.	7 "	23 "
Stud. med. S.	7 "	32 "
Stud. med. L.	3·7 "	13 "
Prof. von B.	7 "	18 "
Stud. med. R.	8 "	19 "
Prof. S.	7 "	17 "
Stud. med. H.	5 "	18 "

Blickt man abwechselnd nach der pendelnden Flamme und nach einem passend abseitsgelegenen Punkt, so scheint das Pendel langsamer und schneller zu schwingen [1] oder doch abwechselnd kleinere und grössere Elongationen zu machen. Um sich von der Rede stehenden Tauschung zu überzeugen, genügt ubrigens jede Hängelampe; ich habe dieselbe zuerst an einer solchen bemerkt.

Of the three experiments, Exner's gives the most satisfactory result. With some practice it is possible to detect very noticeable differences in the rate of motion between the center and the periphery of the retina. It is significant for the view that is presented in this paper that Exner speaks of the actual and estimated *elongation* of the pendulum. It is not the rate at whichthe pendulum travels, but the *distance* covered in its movement which is judged. And in the next to the last sentence in

[1] 'In Analogie zu einem von Collegen v. Fleischel beschriebenen Versuche: Physiol.-optische Notizen, II.,' *Wiener akad. Sitzber.*, Bd. 86, Mai, 1882.

the article it is said that if one looks towards the swinging flame and then away from it, the pendulum appears to move slower (in direct) and quicker (in indirect vision) or to make a *smaller* or *larger* elongation. Doubtless Exner had in mind the physical formula for velocity, in which velocity is directly proportional to space. At any rate it shows that the fact may appear as an enlargement of space or as a quickening of motion. Similarly, Basler speaks of the magnitude of motion (Bewegungsgroesse) without saying whether it is the rate or extent of motion which is overestimated.

I have repeated Exner's experiment with two observers. Not only does the extent of movement appear differently in direct and indirect vision, but the two corresponding halves of the retinas perceive in the same way. The pendulum consisted of a 16 c.p. electric lamp which was suspended from the ceiling of the darkroom by a piece of its own cord one meter in length. The observer sat 50 or 60 cm. from the lamp with his head turned towards it, so that his eye could be directed to one side or the other, or directly at it in such a way that the retinal image of the lamp would fall as desired, upon the temporal, nasal or macular region of the retina. In this manner comparisons were made between the temporal and nasal region of each eye and the fovea: these observations may be called foveal-peripheral comparisons to distinguish them from peripheral comparisons between one peripheral region and another. Exner's observations were all of the foveal-peripheral sort, as are the first four of those in the following lists for each observer. In all observations the observing eye does not move.

Observer.	Eye.	Retinal Regions Compared.	Result.
H. L. Osterud,	R	Nasal and foveal,	Arc longer in indirect vision.
"	"	Temporal and foveal,	" " " direct "
"	L	Nasal and foveal,	" " " " "
"	"	Temporal and foveal,	" " " indirect "
"	R	Nasal and temporal,	" " on the nasal retina.
"	L	Nasal and temporal,	" " on the temporal retina.
C. J. Ducasse,	R	Nasal and foveal,	Arc longer in indirect vision.
"	"	Temporal and foveal,	" " " direct "
"	L	Nasal and foveal,	" " " indirect "
"	"	Temporal and foveal,	" " " " "
"	R	Nasal and temporal,	" " on the nasal retina.
"	L	Nasal and temporal,	" " " " temporal retina.

These results confirm those of Exner. Rate or extent of motion on the periphery of the retina appears faster or larger than on the central region. But it is evident from an examination of the results given above that this statement requires a qualification. Not all peripheral regions give a larger extent of movement than the fovea. In the case of O., the arc when

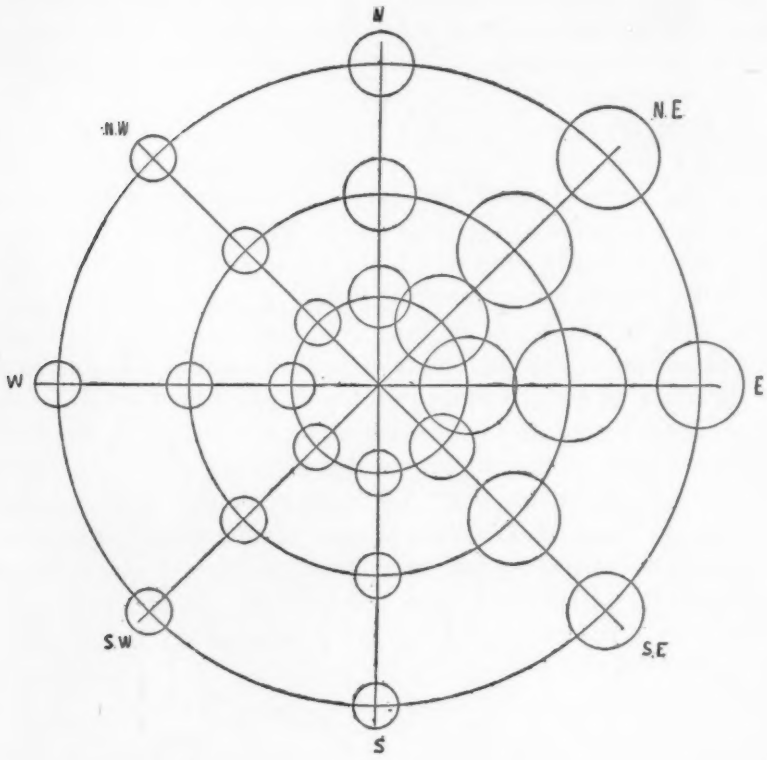

FIG. 1.

seen on the nasal of the right, and the temporal of the left, retina appears larger than when seen on the foveal region. On the other hand when the arc is seen on the temporal of the right and the nasal of the left, retina it appears smaller than when seen on the foveal region. The law should, then, read thus: An extent or a motion will appear longer or faster in indirect, than in direct, vision, when the retinal images of the extent or

moving object are formed upon the left corresponding halves of the retinas; when the retinal images are formed upon the right corresponding halves of the retinas the extent or motion appears less in indirect, than in direct, vision. This statement is confirmed by the results here given (with the exception of the fourth of *D's* observations), and by those previously published.

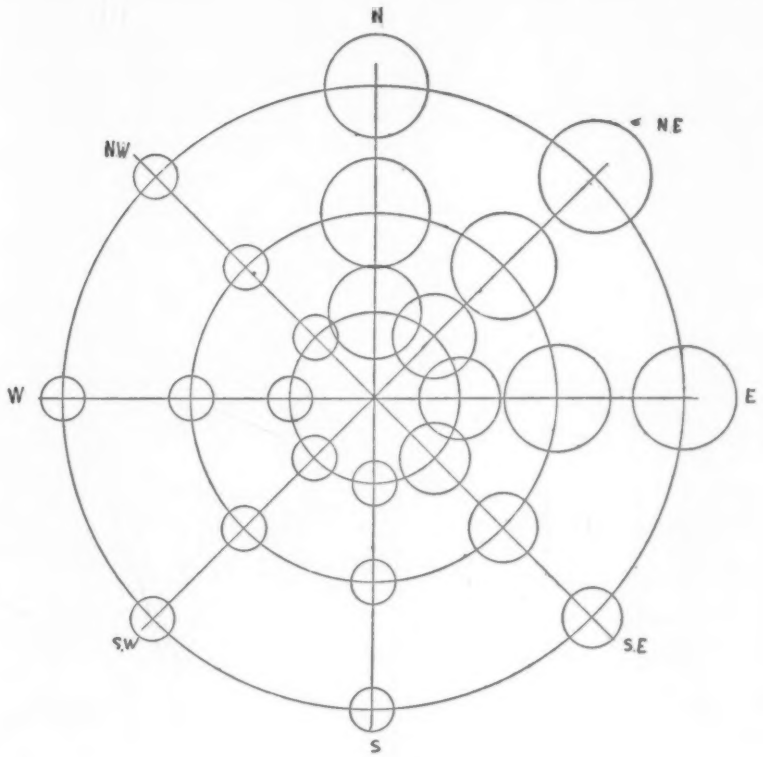

FIG. 2.

Figs. 3 and 4 of the present paper and the same figures of the paper already frequently referred to, show the fact in intuitive form. It is more marked in some observers than in others, but to some extent, it exists in all. The peripheral comparisons show that when the extent or motion falls upon the left corresponding halves of the retinas, it appears larger or faster than when it falls upon the right corresponding halves of the retinas.

The same result may be seen, expressed graphically, in Figs. 1 and 2.

Dresslar's Experiment.— Dresslar's description of his experiment is so brief that it may be quoted in full.

A New and Simple Method for Comparing the Perception of Rate of Movement in the Direct and Indirect Fields of Vision.

While sitting in my room last winter my attention was attracted by the image of a swinging lamp in the mirror, and it then occurred to me that here was a simple method for com-

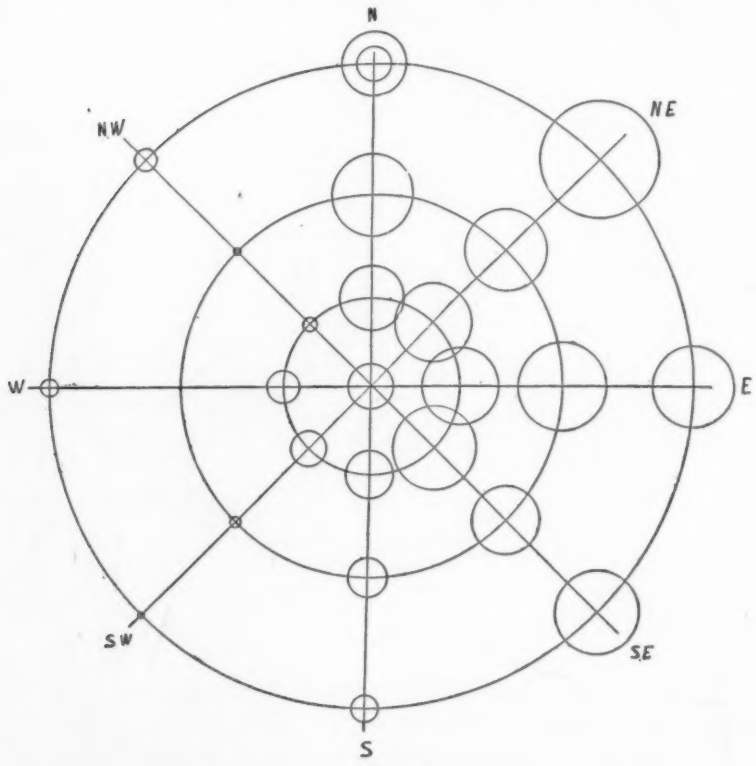

FIG. 3.

paring the perception of rate in the direct and indirect fields of vision. I took a position where, by looking directly at the image in the mirror, the image from the lamp itself fell on the

indirect field of vision. I thus had exactly the same rate, and, provided that I placed myself so that my eye would be near the glass, almost the same extent of movement of the images. The experiment at that time was roughly made, but it showed clearly the well known fact that of two equal rates, the one seen in the indirect field seems to be the more rapid. I have tested the method under more favorable circumstances, and offer it as a simple demonstrational method: Take a small, clear mirror and

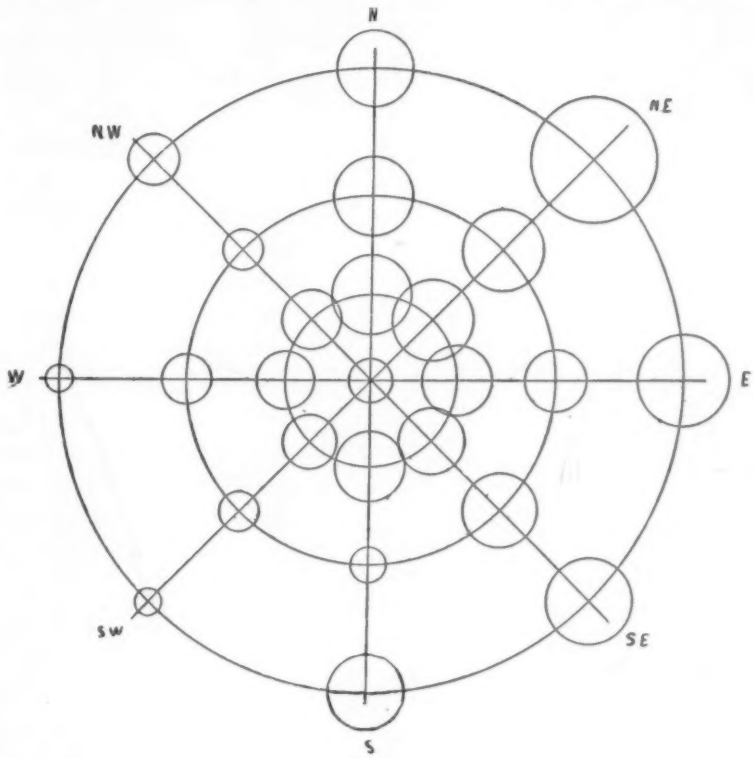

FIG. 4.

arrange it in the median plane immediately between the eyes, so that the eye of the observer may be near the edge of it. Make a pendulum of a small string weighted with a lead ball and place it at some distance away, but near enough to the plane of the mirror to make an angle of perhaps twenty or thirty degrees with it. Swing the pendulum, not too far, nor too fast, in a di-

rection perpendicular to the plane of the mirror. If the observer now directs his eye to some part of the arc, through which the pendulum swings, so that the image of the moving ball will cross the point of clearest vision, the image from the mirror will fall on the indirect field and the two rates can be easily compared. The following diagram will perhaps help to show the arrangement (the diagram is not reproduced). It will be well, perhaps in most cases, to cover the ball of the pendulum with white paper. It should be noticed also that the background should offer no distraction to the attention as sources of error for the judgment. The apparent difference in the rates will be greater when the observer directs his eye toward the pendulum, because the image from the mirror will then fall on the temporal side of the retina, which is less sensitive than the nasal side, especially in an observer whose eyes are deeply set.

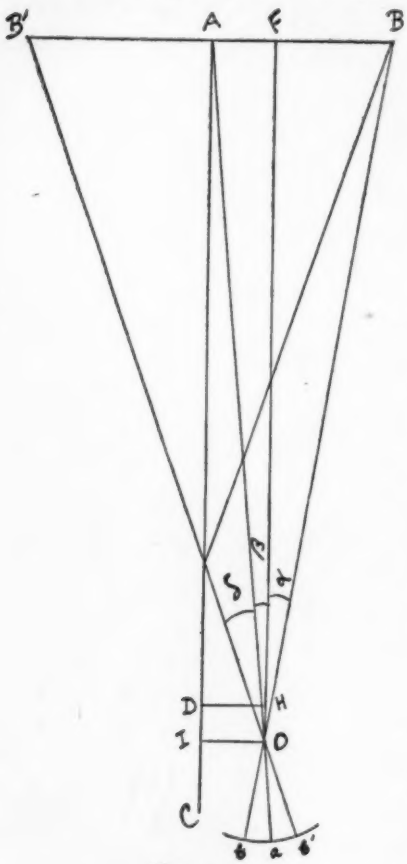

FIG. 5.

I have repeated Dresslar's experiment myself and with two other observers, but with negative results. The fact that the eye which observes cannot be situated in the plane of the mirror would seem to render the experiment useless, since, unless that condition is fulfilled, the retinal arcs of the pendulum and its image in the mirror will be of unequal lengths and, therefore, a prime condition of the experiment violated. Fig. 5 and the trigonometrical considerations which follow from it, show that

with the eye 7 mm. from the plane of the mirror, there would be, under the conditions of our experiment, a difference of 28 minutes in the two retinal arcs. In Fig. 5 the line AC stands for the mirror and the line AB for the pendulum. AB' represents the reflected image of the pendulum and, since the arc of the pendulum is perpendicular to the plane of the mirror, AB is equal to $B'A$. The nodal point of the lens is represented by O and the surface of the cornea by H. The distance between O and H is 7 mm. In the case here considered the line of sight is perpendicular to the arc of the pendulum. In our experiment, $AI = OF = 157$ mm.; $AB = AB' = 40$ mm.; $AF = 7$ mm.; $FB = 33$ mm.; $FB = 47$ mm. Also let $L\gamma = \delta + \beta$. Then

$$\tan \alpha = \frac{FB}{OF} = \frac{33}{157} = 11^\circ\ 52',$$

$$\tan \beta = \frac{AF}{OF} = \frac{7}{157} = 2^\circ\ 33',$$

$$\tan \gamma = \frac{FB'}{OF} = \frac{47}{157} = 16^\circ\ 40'.$$

Hence

$$< \delta = \gamma - \beta = 14^\circ\ 7'$$
$$\alpha + \beta = 14^\circ\ 35'.$$

It is evident that there is a difference of 28 minutes between the arcs subtended by the pendulum and its image in the mirror. Inasmuch as the minimum visibile is about one minute or less,[1] the error from this source is too great to be neglected.

Muensterberg's Experiment. — This experiment is explained in the Milton Bradley Pseudoptics, section D, which is entitled, Illusions of Movement, experiment no. 5. The apparatus consists of a light metal rod 260 mm. in length. At each end there is a square piece of white cardboard 25 mm. on each side. The rod is held in the hand by means of a thread which is attached to the rod in two places. The passage which follows tells how the experiment is to be performed and it also gives an explanation of the result. "Hold the little apparatus horizontally by

[1] Kuelpe, *Outlines of Psych.*, Eng. tr., 358.

the thread and swing it gently from side to side, rather close to the eye and follow the movements of one of the white moving squares. The other square seen with the side part of the eye in indirect vision seems to move stronger and quicker than the one at which you look, and it appears to become slower as soon as you look at it directly. Explanation of experiment no. 5. The side parts of the retina which are less sensitive to color and to form are more sensitive to motion and to change." It is impossible for me to obtain a satisfactory result with this apparatus. Two serious errors are involved: (1) The eye follows the moving square. That means that the image of the object falls on the same retinal locality throughout the experiment. The motion which is felt is due to the moving of the eye and not to the moving of the object over the stationary retina, which is the proper condition for the observation. (2) The movement of the peripheral square is much more noticeable than that of the other, for the reason that the motion of the central square is greatly foreshortened by being directly looked at.

II. Our Own Experiments and Results.

Our own experiments were carried out with the perimeter and moving discs to ascertain whether the illusory differences in rate of motion were distributed in the visual field in a manner similar to the illusion of size. The assumption was that, if there be a complete coincidence of the size and rate illusions, as regards their occurrence in the visual field, one must be the cause of the other. As the apparatus with which the experiments were made was the same as that with which the experiments on the space values of the peripheral retina were made, I must refer the reader to that paper for particulars as to method and dimensions. Here it will suffice to say that when the white discs bearing theiblack spots appeared at symmetrical points in the field of vision, the orbit of one spot seemed larger, or the spot itself seemed to travel faster, than the other. The discs were so adjusted on their spindles that the spots always occupied symmetrical posir tions, although the direction of motion of the discs, as has been explained, was opposite. That is to say, if, at any time, the motor was stopped, and the two discs superposed face to face,

the two spots would overlap. It will thus be seen that the observer was asked to compare the rates of two spots moving at uniform speed, in circular orbits, in opposite directions. The rate of revolution was about one per second. Seen under these conditions, the orbit of one spot seems larger, and the spot itself seems to be moving faster, than the other. The problem was to determine whether the orbit appears larger because the spot is traveling faster, or whether the spot appears to be traveling faster because the orbit is larger. Two facts point to the second alternative: (1) Wherever, in the visual field, the orbit appears larger, there the rate of the spot appears faster. And whenever the apparently larger orbit is diminished, as it may be by pushing the spot towards the center of the disc, the difference in rate disappears as the orbits become subjectively equal. (2) The difference in size is as apparent with motionless objects (cardboard discs, for example) as with the moving spots. I give, therefore, the results of experiments with two observers, in which the distribution of the size illusion is shown both in numbers and in figures. That the faster motion always concurred with the apparently larger orbit will have to be accepted on the strength of the statement, as I have not been able to devise any means of measuring the motion independently of the size.

The experiments were either comparisons between two peripheral regions of the retina of one eye, as between the nasal and temporal region of the right eye, or between the fovea and some part of the periphery, of the retina of the same eye. The former sort are called peripheral, the latter, foveal-peripheral, comparisons. All of the experiments are quantitative. That is to say, the exact amount of apparent difference between one retinal locality and another, was measured. The measurements were made by shortening the radius of the orbit of the apparently larger disc, until it equalled, subjectively, the orbit of the other. The actual reductions, for each position in the visual field, are given in millimeters, in Tables I. and II. As the perimeter describes in revolution a hemisphere, of which the pole is the fixation point, the various positions in the visual field, which were studied, may be conveneintly described by reference

to the meridians and parallels of this hemisphere. The positions fall upon four meridians: the vertical, horizontal and two oblique, 45 degrees from the vertical, in the first and third, and in the second and fourth quadrants. The parallels were ten,

TABLE I.

Meridian.	Parallels.		
	25	20	10
N. & S.	Re 4 Le 4	Re 8 Le 5	Re 3 Le 2
		Re 5 Le 6	
	Re 4 Le 2	Re 5 Le 4	Re 3 Le 1
		Re 6 Le 4	Re 5 Le 7
N.E. & S.W.	Re 8 Le 8	Re 15 Le 15	Re 11 Le 12
	Re 15 Le 17		Re 9 Le 10
E. & W.	Re 10 Le 5	Re 15 Le 14	Re 13 Le 14
	Re 14 Le 5		Re 9 Le 9
	Re 14 Le 8		
S.E. & N.W.	Re 9 Le 5½	Re 10 Le 10	Re 3 Le 3½
	Re 6 Le 6		Re 6 Le 5

TABLE II.

Meridian.	Parallels.		
	25	20	10
N. & S.	Re 14 Le 13	Re 14 Le 15	Re 11 Le 11
N.E. & S.W.	Re 15 Le 16	Re 14 Le 14	Re 8 Le 10
			Re 11
E. & W.	Re 13 Le 14	Re 13 Le 16	Le 11 Re 6
			Le 6
S.E. & N.W.	Re 4 Le 3	Re 5 Le 6	Re 8 Le 8
			Re 6 Le 2

twenty and twenty-five degrees from the fixation point or pole. The meridians are further designated as north and south for the vertical and east and west for the horizontal; the others receiving likewise appropriate names from the points of the compass. In all of the experiments, but one eye was used at a time and that eye was always fixed upon the pole of the visual hemisphere. In the case of the foveal-peripheral comparisons, when one disc covered the fixation point, the eye rested upon the center of the disc.

TABLE III.

Meridian.	Parallels.		
	25	20	10
N.	Re 6 Le 2 Re 2 C Le 3 C	Re 8 Le 8	Re 4 Le 4
N.E.	Re 17 Le 15	Re 9 Le 7	Re 7 Le 7
E.	Re 8 Le 8	Re 8 Le 10 Re 10 Le 10	Re 8 Le 7 Re 7 Le 6
S.E.	Re 11 Le 6	Re 6 Le 6 Re 4 Le 4	Re 8 Le 9
S.	Re 4 C Le 4 C	Re 2½ C Le 0 C	Re 0 Le 1
S.W.	Re 8 Le 9	Re 9 C Le 7 C Re 7 C Le 8 C	Re 2 C Le 2 C
W.	Re 6 C Le 6 C	Re 10 C Le 9 C	Re 3½ C Le 3 C
N.W.	Re 5 C Le 5 C	Re 7 C Le 7 C Re 10 C Le 10 C	Re 7 C Le 7 C

The peripheral comparisons are given for observers, *O* and *D* in Tables I. and II.; the foveal-peripheral comparisons for the same observers in Tables III. and IV. The tables show the amount of enlargement a disc undergoes in any of the positions in the visual field, which was studied. For example, in Table

II., in the horizontal meridian, 25° from the fixation point, it was found necessary to reduce the right orbit by 13 mm. for the right eye, before the right, appeared equal to the left disc. In the peripheral comparisons, the right disc in the visual field always appeared larger than the symmetrically placed disc in the left half of the field, and the upper disc always appeared larger than the lower disc. The figures in the tables tell how much the larger, had to be reduced to equal the smaller, disc. In the foveal-peripheral comparisons, the peripheral disc appeared larger in some positions while in others, the central disc ap-

TABLE IV.

Meridian.	Parallels.		
	25	20	10
N.	Re 7 Le 8	Re 8 Le 8	Re 8 Le 8
N.E.	Re 19 Le 19	Re 8 Le 9	Re 9 Le 8
E.	Re 11 Le 11	Re 3 Le 5	Re 6 Le 6
SE.	Re 10 Le 9	Re 7 Le 7	Re 5 Le 5
S.	Re 7 Le 8	Re 2 C Le 2 C	Re 6 Le 6
S.W.	Re 4 C Le 4 C	Re 0 Le 2 C	Re 3 Le 1
W.	Re 5 C Le 3 C	Re 2 Le 0	Re 3 Le 3
N.W.	Re 2 Le 0 Re 2 Le 2	Re 1 C Le 1 C	Re 3 Le 4

peared larger. In the tables, the letter, *C*, indicates that in those positions the central disc appeared larger than the peripheral disc by as many millimeters as are indicated by the numbers. When the central disc appeared larger, it was diminished until it equalled the peripheral disc. The figures (1, 2, 3, 4) express in graphic form the results of the observations. The concentric circles stand for the parallels, and the straight lines for the meridians, of the visual field. At the intersection of the parallels

and meridians the amount of the enlargement is represented. The enlargement is represented by assigning to the circle which represents the smaller disc, a certain arbitrary radius which in the plates was 10 mm. The circle which represents the larger disc is made with a radius of 10 mm. plus the number of mm. by which the orbit of the larger disc was actually decreased. The number of mm. taken as the amount of increase is the average of all the readings taken, for both right (Re) and left (Le) eyes. In some cases determinations of the same positions were made several times after an interval of a month. In such cases all of the determinations are included in the appropriate square of the tables. Two discrepant readings for the same position are represented, in the figures, by two concentric circles. There is one case of this in Fig. 3.

It should be understood that the view here presented with regard to the cause of the illusory increase of speed on the periphery of the retina, does not invalidate the theory that motion is felt as a sensation and not as a perception of change between two points. The experiments first pointed out by Exner, that the fingers may be seen to move in the periphery of the field of vision where they cannot be discriminated as two or more and the similar observation that the movement of the points of a pair of compasses within an area in which the points cannot be felt as two, prove only that the feeling of motion does not depend upon the discrimination of two points. Basler's experiment is even more significant for this view, for he found that a motion between two points may still be felt although, on the on the same place on the retina in direct vision, the two points could not be discriminated as separated.[1] Basler assumes a peculiar arrangement of the rods and cones of the retina, by means of which two retinal elements may be excited, although the lateral displacement of the moving object is less than the diameter of one rod or cone. Basler's fact lends weight to the sensational view of motion, while his theory seems to imply that two termini in the shape of two *different* sense organs, are necessary to the perception of motion. The results of our own experiments show that the feeling of motion is dependent upon

[1] *Loc. cit.*, 587.

the feeling of space, although not upon the capacity to distinguish two points as separate. That the feeling of extent and the discrimination of two points are different kinds of spatial feeling has already been urged by Wundt.[1] He points out that, on the back, where the discrimination is poor, just as soon as the threshold is passed the extent is felt as a relatively large one, while on the finger tip where the discrimination is much finer, the distance is felt to be small when once the threshold is exceeded. Exner is right in combating the theory of Vierordt that a *terminus a quo* and a *terminus ad quem* is necessary to the feeling of motion. But if he means, in saying that motion is felt as a sensation, that, as such, it is not dependent upon extent, the present writer must disagree with him.

Summary.

1. There is a fundamental difference in the perception of size between the right and left corresponding halves of the retina. When stated in terms of the retinas, the fact is this: Objects whose retinal images are formed upon the nasal retina of the right, and on the temporal retina of the left, eye appear larger than similar objects whose retinal images are formed upon the temporal retina of the right, and on the nasal retina of the left, eye. When stated in terms of the field of vision, the fact is this: Objects seen in the right half of the visual field appear larger than similar objects of the same size in symmetrical parts of the left half of the visual field.

2. This difference in space perception is the cause of the apparent increase in rate which moving objects, seen in indirect vision, seem to undergo. The evidence for this view rests upon two points: (1) The distribution in the visual field of the size illusion is exactly co-extensive with that of the rate illusion. But if there were nothing more than this similarity of distribution, either one might be the cause of the other. The space illusion is fundamental for the reason that (2) the differences in space perception are apparent with discs of cardboard which are motionless.[2]

[1] *Physiol. Psych.*, II., 442 and 512.

[2] The MS. of this article was received August 7, 1908. — Ed.

A STUDY OF GALVANOMETRIC DEFLECTIONS DUE TO PSYCHO-PHYSIOLOGICAL PROCESSES.[1]

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PART I.

I.

The purpose of our present study is to investigate the relation of emotions and physiological activities to galvanometric deflections. Our aim is to ascertain whether galvanometric deflections can invariably be correlated with psycho-physiological processes. We attempted to eliminate all errors and obviate objections incident in the carrying out of such delicate work and also to deal with the problem of causation. This communication should be regarded as a preliminary one; further research is in progress.

In his study of the emotions Ch. Fèrè was the first to point out the presence of electrical changes under the influence of emotions and affective states in general. He regards the changes as the result of lowering of bodily resistance brought about by the emotions and their correlative physiological processes. R. Vigoroux followed up the subject by a study of clinical cases and referred the electrical changes found not to skin resistance, but to modifications of capillary circulation under the influence of emotions.

In experimenting with the mirror-galvanometer Tarchanov found galvanometric perturbations due to emotions, affective states, sensations and ideas; in fact, according to him, all kinds of mental states bring about deflections of the galvanometer. He describes large deflections apparently caused by the slightest

[1] The MS. of this article was received May 13, 1908. — ED.

emotion and change of affective states and even by mere memory and representation of an emotion. In fact, according to Tarchanov, even such intellectual processes as calculation give rise to marked deflections of the mirror-galvanometer. As the result of experiments which have been repeated by us in the present research he came to the conclusion that the galvanic phenomena manifested were due to secretory changes of the skin. It is to be regretted that Tarchanov did not follow up his experiments as he had promised to do in his original paper.

Many other investigators such as Stricker, Sommer, Müller, Veraguth and more recently Jung, have taken up the subject and have advanced different views and hypotheses as to the causation, physiological and physical, of the galvanic phenomena in relation to psychic activities. While Stricker in opposition to Tarchanov rejects the theory of skin-effects and variations of the activity and secretion of sudorific glands, but ascribes the galvanic phenomena to circulatory changes under the influence of mental states, Sommer regards the phenomena more in the nature of an artefact. He thinks that the galvanic deflections are simply the result of variations of contact between skin and electrodes and that they are due to the movements of the hand, voluntary or involuntary, and to changes of pressure exerted on the electrodes. This is a very serious objection which must be overcome before we can establish any relation between mental processes and observed galvanometric perturbations.

Recently Veraguth and after him Jung have devoted much of their time and energy to the study of the relation of mental states to galvanometric deflections. Veraguth confirms Tarchanov's results and terms the galvanic phenomena observed by him under the influence of emotions and of mental states in general, as 'the psycho-physical galvanic reflex.' Jung has been especially energetic in calling attention to the galvanic phenomena of emotional disturbances and has attempted to utilize them in the detection and analysis of suppressed systems and possibly also of dissociated subconscious mental states. Peterson and Ricksher, working in Jung's laboratory, have in their investigations, performed on normal and on insane individuals, further corroborated the relation between

galvanometric changes and mental states in general. Large galvanometric fluctuations were found by them to be correlated with sensory, emotional and ideational system-complexes. The galvanic perturbations due to ideo-sensory states are referred to the affective states or 'feeling tone,' which, according to Wundt's doctrine, widely accepted in Germany, are supposed to accompany all mental processes.

The following table [1] may possibly give some concrete idea of the magnitudes, as found by these observers, of the galvanometric deflections under the influence of mental states:

Stimulus.	Deflection of Galvanometer.
Multiplying 4×4	34 cm.
Multiplying 9×11	18
Call by name	5
Where do you live?	38
Capital of Switzerland?	14
How old are you?	24
Are you married?	18
Were you engaged before?	27
Have you been a nurse before?	26
Imagined threat of prick with needle	4.3
Imagined threat of fall of heavy weight	1.6
Imagined grief	2.8
Thought of an amusing story	1.8
Thought of a painful illness	1.6

An inspection of the table shows that the magnitudes of deflections vary greatly among themselves without apparent corresponding variation of the stimuli. Our results differ from these as the sequel will show.

II.

The electrical circuit and apparatus employed by us for these experiments were as simple as possible, in order that there should be no doubt as to the origin of the effects observed. The arrangement was as indicated in Fig. 1.

In series with a battery B was a sensitive galvanometer G and two electrodes EE, across which the subject placed himself, thus closing the circuit. The battery was a single cell giving a constant electromotive force of about 1 volt, which was some-

[1] Peterson and Jung, 'Psychophysical Investigations,' *Brain*, 1907.

times replaced by a thermo-element giving only a few millivolts, and sometimes entirely removed from the circuit. The galvanometer was of the suspended coil, D'Arsonval type, and of extreme sensitiveness. The deflections were read by means of a beam of light deflected from a mirror *M*, attached to the moving coil of the instrument, to a telescope *T* with a scale *S*. A deflection of 1 cm. on the scale corresponded to less than 10^{-9} ampère

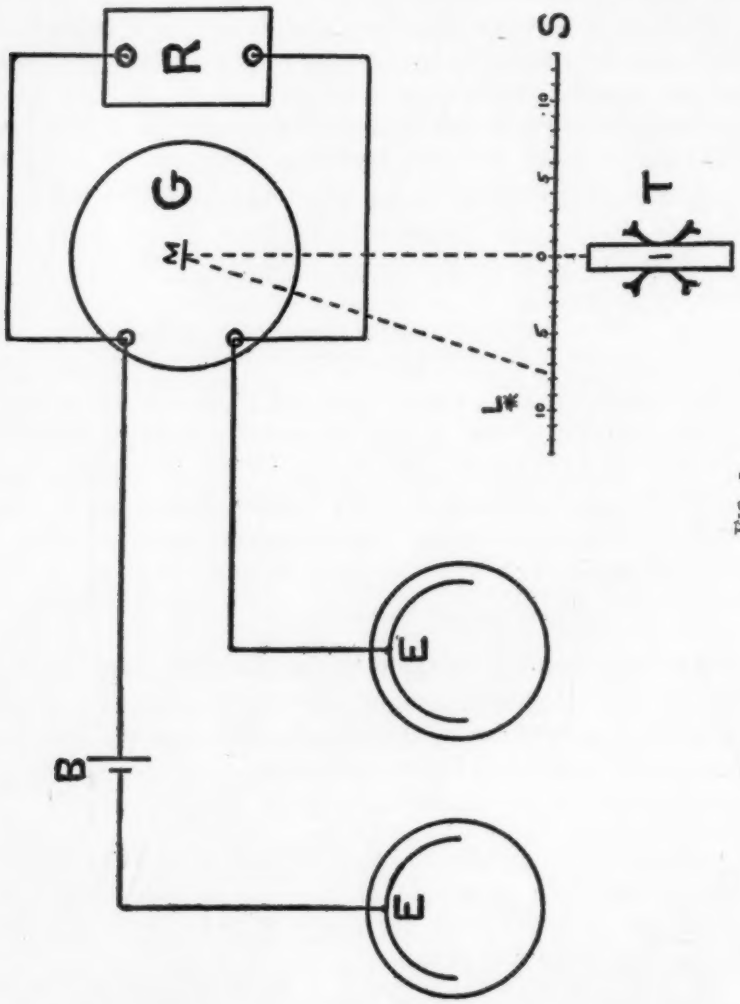

FIG. 1.

through the instrument. This extreme sensitiveness was too great for many of our early experiments, so that a resistance R, which could be varied to reduce the sensitiveness to any desired degree, was shunted around the galvanometer.

Not only is it important that the galvanometer employed should respond to very small changes in the current flowing through the circuit, but it must respond quickly, that is, without appreciable lag. The speed of reaction of the galvanometer was tested by direct experiment. First, with an all copper circuit, and then with the human body across the electrodes EE, the circuit was made and broken at the rate of 200 times per minute. Each change was definitely observed by corresponding galvanometric deflections.

The electrodes EE were glass vessels of about 4 liters capacity, nearly filled with a strong electrolyte, as for instance a concentrated solution of NaCl. Into these vessels large copper electrodes C of about 500 $\overline{\text{cm}}.^2$ area were permanently placed. The circuit was completed by placing the hands, feet, etc., one into each electrode solution.

The galvanometric deflections may be due to changes in the resistance at the electrodes, brought about by such purely physical causes as motion or muscular contraction of the hand, stirring of the electrode fluid or similar incidental secondary effects. In order to eliminate the possibility of such effects it was necessary to devise such electrodes that the current through the circuit should within very wide limits be independent of the position of the hands. The possible sources of error at this point which would change the effective surface of the hand are twofold—(1) due to the variation of the liquid level at the wrist, and (2) due to movements of the hand as a whole. The following device was used to overcome these difficulties. The wrist was covered with shellac for a length of several inches, so that the free liquid surface of the electrode was always in contact with shellac. The shellac was covered by a layer of paraffin, though a moderate coating of shellac alone was such a good insulator that the electrode resistance became independent of the height of liquid on the wrist. In addition to this the hand was put in splints in such a manner that only a small fraction of the

skin was covered, so that no appreciable muscular contraction of the phalanges could take place.

Under these conditions the subject could move his hands about at will through a distance of several centimeters, stirring the electrode liquid violently — the equilibrium reading of the galvanometer remaining absolutely undisturbed. If now a stimulus was given which aroused an emotion or definite affective state of the subject, a marked galvanometric deflection was observed.

A large series of experiments was performed under these conditions.

(*To be concluded.*)

DISCUSSION.

CONSCIOUSNESS AND MEANING.

In the interest of better understanding I wish to comment on Professor Bode's discussion in the July issue of the REVIEW, so far as he refers to my own views on the nature of consciousness.

Professor Bode states the general problem tersely, it seems to me, when he asks, "When an object becomes known, what is present that was not present the moment before?" I have attempted to answer that question in one word — "meaning." This answer may be incorrect or inadequate, but I do not see that it forces the maker into a dilemma. When objects become known, they do not — as I am inclined to think — lose any of the properties or relationships they had before. Water does not become water or cease to be water when I become conscious of it. But when objects become known, they then mean something. The water which was water before I was conscious of it and is still water when I am conscious of it, is now, when I am conscious of it, water meaning something, the quenching of thirst, for example.

Why, then, does Professor Bode state: "Either a fact upon becoming a fact of consciousness transforms itself from an unknowable something into a relation of meaning or it undergoes no such transformation. In the former case the distinction between what is consciousness and what is not is inept, since all is consciousness; while in the latter we have again on our hands the difficulty that no differentia are furnished whereby a given personal experience is distinguished from other facts." Assuredly when an unknown fact becomes known, it has changed from an unknown to a known fact, and to that extent has changed its character. But the question is as Professor Bode has put it, What is present with the known fact which was not present with the unknown fact? Take the water again. When it is unknown — meaning thereby when it is not an object in consciousness — is it lacking in particular chemical and physical properties? Does it take on these properties — or lose them — only in consciousness? If it can have these properties both in consciousness and out of it, there is no necessity of adding anything to them to make them just those properties when they are in consciousness. On the other hand, it seems very difficult to believe that water could mean anything and not be in consciousness; and it seems very natural to believe that if water should

come to mean anything it would, by that fact, have entered consciousness.

And now a word about 'awareness,' for one may claim that not only must a fact mean something to be in consciousness, but we must also be 'aware' of the fact as fact and also of the fact as meaning something. I repeat what I have already said, but now in general terms. If a fact has certain properties which it neither acquires nor loses by virtue of its presence in consciousness, it is evidently unnecessary to add anything to those properties to make them the same properties in consciousness that they are when out of it. Consequently it is not necessary to add 'awareness.' That is, it is not the 'awareness' of a fact that makes the fact a fact of a particular kind in consciousness; it is that in its own right. The simple existence of the fact in consciousness appears, therefore, to be all that is involved in 'awareness of the fact as fact.' But let the fact mean something and its meaning appears to be identical with 'awareness of it as meaning something.' Or, to state the situation in the form of a paradox: If objects were in my consciousness, but entirely devoid of meaning, I should not be aware of them. The presence of meaning among the facts is the awareness of them. In short — to drop the paradox — 'awareness' is but another term for 'consciousness.'

The position which I have thus briefly outlined seems to me to answer Professor Bode's initial question directly and unambiguously. I do not say it answers it correctly or adequately. That is matter for consideration. But I do not see how it can be reduced to the dilemma he states in the discussion of it.

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